THE
GLACIAL NIGHTMARE
AND THE FLOOD

H. H. HOWORTH
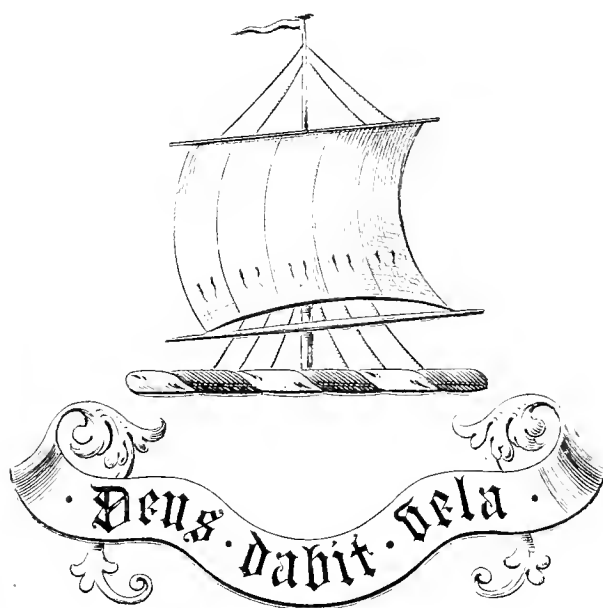
THE GLACIAL NIGHTMARE
AND THE FLOOD.

VOL. II.

THE

GLACIAL NIGHTMARE

AND THE FLOOD

A SECOND APPEAL TO COMMON SENSE
FROM THE EXTRAVAGANCE OF
SOME RECENT GEOLOGY

BY

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ETC., ETC.

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THE GLACIAL NIGHTMARE.

CHAPTER X.

APPEALS TO TRANSCENDENTAL METEOROLOGY.

Simple naturaliste, je me trouve mal à l'aise au milieu de ces hypothèses contradictoires qui échappent au contrôle direct de l'observation et de l'experience. L'ancienne extension des glaciers est un fait ; la découverte des causes que l'ont produite sera l'honneur des futures générations scientifiques."—C. MARTINS, Revue des Deux Mondes, lxviii. 223.

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WHEN astronomy failed to help the glacial geologist in his quest for some cause which should explain the tremendous problem which he had created for himself, and which he had begun to fancy something more than a hypothesis, he turned to a

more difficult and less rigorous science, which is still in its youth, and which is still, and perhaps will always remain, based upon less certain elements—a science in which there is consequently much room for subtle speculation, and for ingenious suggestion.

One of the earliest meteorological theories produced to account for the glacial period was that suggested by the Swiss naturalists, and notably by Escher von der Linth, namely, that the former extension of the Alpine glaciers was due to the Sahara having once been a sheet of water, the evaporation from which caused a humid atmosphere to prevail where the snow-eating Föhn now blows. This view had two insuperable difficulties to contend against. In the first place, the glacial phenomena are very widespread, and whatever effect replacing the Saharan sands by a sea might have upon the Alpine glaciers, it is clear that it could have little upon Scandinavia and Finland, and none at all upon North America. Secondly, it takes for granted the notion which once prevailed very widely, and which I myself urged in my volume on the Mammoth, that the Föhn comes from the Sahara. As a matter of fact it has been shown by very careful observations to have nothing to do with the Sahara. Dove was the first to show that the Föhn, instead of coming from Africa, comes in fact from the Caribbean Sea, and that Europe is the chief condenser for the vapour which arises there.[1] His views have been further developed by Mühry, Wild, and other meteorologists.[2]

We may therefore put aside this theory as incompetent to explain the facts.

Professor Schmick propounded a new theory as to the cause of the glacial period in a memoir entitled " Die Umsetzung der Meere und die Eiszeiten der Halbkugeln der Erde, etc.," Köln 1869, and in various subsequent papers. This was welcomed very enthusiastically in some quarters. *Inter alios*, Professor K. Völker, in his "Kosmogenishe Vorträge," Schaffhausen 1872, p. 139, speaks of it as doing for geological speculation what Copernicus had done for astro-

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[1] See Dove, Ueber Eiszeit, Föhn und Sirocco, p. 10.
[2] Pilar, Ein Beitrag zur Frage ueber die Ursache der Eiszeiten, 15, 16.

nomy. Schmick argued in favour of a secular fluctuation in the sea level, and the corresponding shifting of the warm zones to the north and south of the equator, thus causing an alternate glaciation in each hemisphere. He would seem to urge that, owing to the variation in the eccentricity of the earth's orbit combined with precession, there is an alternate attraction of the waters of the earth to either hemisphere, due to the sun having $\frac{1}{30}$th more power over them when acting on the hemisphere which has its summer in perihelion. At present this is exercised upon the southern hemisphere, and this has been so for 5871 years, and will remain so for another 4629 years, completing the hemicycle of 10,500, when the process will be transferred to the northern hemisphere. Professor Schmick's views have been critically examined by Pilar and others, and shown to be based upon quite transcendental and impossible astronomical notions.

Among other things, Schmick attributes to the sun a kind of lateral traction upon the waters of the ocean, while as a matter of fact its effect is vertical. The tidal wave does not translate water from one area to another, it merely raises up the water in a moving mound with a furrow on either side. The notion that water can be translated by means of a tidal wave is a complete mistake; the wave passes on, but the water, after it has passed, remains where it was. Again, it is true that in times of greater eccentricity the sun will attract the water more than at other times, while the earth is in perihelion, but the fact of the nearness of the sun and its consequently increased pulling power is compensated by the greater speed of the earth under these conditions. And there can be small doubt that the total drag of the sun upon the hemisphere which faces it in perihelion, is the same as the corresponding drag upon the other hemisphere during the longer time when it is directly acting upon it. If, by the operation of the sun's dragging power, a greater quantity of water is accumulated on the southern hemisphere in one season, the sun's dragging power, less in amount but continuing longer, will pull the same quantity of water to the other hemisphere in the other and longer season. So that there will be no permanent accumulation on either hemisphere, and no shifting of the warm zones.

Dr. Schmick's arguments in regard to the uniform sinking of the waters in the northern hemisphere in historic times are met by the fact that there is no such uniformity. In the north of Sweden the land rises faster than it does farther south; so in Greenland; and in both these districts the water is sinking in their most southern portions, and so with the coasts of the Mediterranean, etc. In fact, as Pilar says, Schmicks' theory is untenable, both physically and mathematically, and is at issue with that of gravitation; or as Professor Bruhns, a mathematician of repute, says, " His theories are at issue with the laws and theories established by Kepler, Newton and Laplace."[3] We need not, I think, discuss them further.

A more reasonable, and at the same time more widely accepted view, was that of the French mathematician, Adhémar. He published a well-known work in 1840 entitled, "Revolutions de la Mer." In this he admits fully, that both hemispheres receive the same quantity of heat from the sun in each year, whatever the eccentricity of the earth's orbit may be, as was proved by D'Alembert long ago, but he argues that, although they receive the same amount of heat, one loses a great deal more than the other by radiation; this being due to the fact that the winter in one hemisphere is longer than in the other.

Adhémar's main position depends upon a principle first pointed out by Humboldt in his memoir on " Isothermal Lines and the Distribution of Heat over the Globe." He argues that although it is true the total heat received by the earth is evenly distributed between the two hemispheres, yet the temperature of the globe is dependent not merely on the heat which it receives, but also upon the heat which it radiates into space; and it is argued that in the case of radiation there is no compensation between the two hemispheres, but the reverse. " The southern hemisphere, it is asserted, has not only a colder winter than the northern in consequence of the sun's greater distance, but it has also a longer winter, and the extra loss of heat from radiation in winter is not compensated by its nearness to the sun in

[3] Pilar, op. cit. 32.

summer, for it gains no additional heat from this proximity; and in the same way it is argued that as our winter in the northern hemisphere, owing to the less distance from the sun, is not only warmer than that of the southern hemisphere, but is also at the same time shorter, our hemisphere is not cooled to such an extent as the southern. And thus the mean temperature of the winter half-year, as well as the intensity of the sun's heat, is affected by a change in the sun's distance."

Following up this view, Adhémar argues that, "The south pole loses in one year more heat than it receives, because the total duration of its night surpasses that of its day by 168 hours; and the contrary takes place for the north pole. If, for example, we take for unity the mean quantity of heat which the sun sends off in one hour, the heat accumulated at the end of the year at the north pole will be expressed by 168, while the heat lost by the south pole will be equal to 168 times what the radiation lessens it by in one hour; so that at the end of the year the difference in the heat of the two hemispheres will be represented by 336 times what the earth receives from the sun, or loses by radiation in an hour." [4]

This argument has been met by Dr. Croll with his usual acuteness. He urges that, inasmuch as the rate of radiation depends on the rate of absorption of heat by the earth, and as the southern hemisphere receives in winter exactly the same amount of heat as the northern, but distributed over a longer time, it must radiate this heat more slowly; and the less rate of radiation would compensate for the longer period, and equalize the total winter loss of heat in each hemisphere, just as in its shorter and fiercer summer the radiation would be quicker, and thus also equalize the loss—but for one fact. This is that, owing to the slow conductivity of the ground for heat, a greater quantity of heat will pass into it during the long summer of aphelion than during the short one of perihelion, and similarly more heat will pass out of it during the long winter in aphelion than during the shorter one in perihelion, for the amount of heat traversing the ground and the amount absorbed depends on the length of time during which the heat is passing, as well as

[4] Revolutions de la Mer, 2nd ed. 37. Croll's Climate and Time, 81, 82.

upon the amount of heat itself. And although the higher temperature of the surface, if that surface were hotter, would induce a greater rate of absorption, as its lower temperature would tend to lessen its radiation into space, yet in neither case would these fully compensate for the shortness of the hot period, and the length of the cold period respectively. The general and combined result of these causes would be that in case the two hemispheres were contrasted, as they are now, in regard to the length of their seasons, a slight accumulation of heat would take place on the northern hemisphere, and a slight loss on the other. But this loss of heat on the one hemisphere and gain on the other would not go on accumulating at a uniform rate year by year, as Adhémar supposes. Croll goes on to make an elaborate investigation of the actual amount of heat stored up by one hemisphere and lost by the other; and taking the mean of the results from the three substances, sandstone, sand, and trap, he finds that the total amount of heat above the mean temperature stored during the summer would, if applied to water, be equal to raising a column thirty-one feet three inches in height 1° C. He next calculates that allowing 22 per cent. for the amount cut off by passing through the atmosphere, the amount of heat received from the sun in two days and ten hours in the latitude of Edinburgh would be equal to the same work; and it follows that the total quantity of heat stored up during summer in the latitude of Edinburgh is only equal to what is received there from the sun during two days and ten hours at the time of the equinoxes. Three days' sunshine during the middle of March or September, if applied to raise the temperature of the ground, would restore all the heat lost during the entire winter, and another three days' sunshine would confer on the ground as much heat as is stored up during the entire summer. It must also be remembered that the total duration of sunshine in winter is to that of summer in the latitude of Edinburgh only about four to seven, a difference of two months; while the amount of heat received is scarcely one-third, yet the ground in winter loses only about six days' sun-heat below the maximum amount possessed by it in summer.

The amount of heat lost in winter, again, is not chiefly owing to radiation, but to a decrease in the quantity of heat actually

received by the earth in consequence of the sun's longer absence, combined with the obliquity of his rays during that season.

Supposing, however, we attribute all the loss to radiation, then, inasmuch as the excess of the winter nights over those of summer is in the proportion of seven to four, that is, a difference of 1200 hours, and if we calculate that this gives a loss of three days' sunlight, we have a measure to apply to the excess of the south polar winter over the north, which amounts to about 184 hours, and gives an actual loss of only five and a half hours' sunshine.

"It is no doubt true," says Croll, "that the two cases are not exactly analogous, but it is obvious that any error which can possibly arise from regarding them as such cannot materially alter the conclusion at which we have arrived. Supposing the effect were double or even quadruple what we have concluded it to be, still it would not amount to a loss of two days' heat, which would certainly have little or no influence on climate."[5]

In all this reasoning the earth has been treated as a simple absorber and radiator of heat. No account has been taken of the distribution of sea and land, the movement of currents of water and wind and other modifying causes. Everything also is assumed to be the same in both hemispheres, except that the winter of one hemisphere is taken as longer than that of the other. That is, the case has been taken as favourably for Adhémar as possible.

"But even assuming," says Dr. Croll, "that all the preceding reasoning is incorrect, and that the southern hemisphere, in consequence of its longer winter, loses heat to the extravagant extent of 168 hours supposed by Adhémar, still this could not materially affect climate. The climate is affected by the mean *temperature* of the *surface* of the ground, and not by the quantity of heat or cold that may be stored up under the surface. . . . Underground temperature can only affect climate through the surface. If the surface, for instance, could be kept covered with perpetual snow, we should have a cold and sterile climate, although the temperature of the

[5] Climate and Time, 83-86.

ground under the snow was actually at the boiling-point. Let the ground to a depth of, say, forty or fifty feet, be deprived of an amount of heat equal to that received from the sun in 168 hours. This could produce little or no sensible effect on climate; for, owing to the slow conductivity of the ground for heat, this loss would not sensibly affect the temperature of the surface, as it would take several months for the sun's heat to penetrate to that depth and restore the lost heat. The cold, if I may be allowed to use the expression, would come so slowly out to the surface that its effect in lowering the temperature of the surface would scarcely be sensible; and, again, if we suppose the 168 hours' heat to be lost by the mere surface of the ground, the effect would certainly be sensible, but it would only be so for a few days. We might in this case have a week's frozen soil, but that would be all. Before the air had time to become very sensibly affected by the low temperature of the surface, the soil would be thawed. The storing up of heat or cold in the ground has in reality very little to do with climate. . . . It is therefore, I presume, quite apparent that Adhémar's theory fails to explain why the southern hemisphere is colder than the northern." [6] This ingenious and able analysis of Adhémar's theory shows, conclusively, that it will not bear a critical sifting, and is in fact an altogether inefficient cause of serious climatic change.

I now propose to criticize a theory which dominates a large number of scientific men and is the foster-mother of much additional speculation, I mean Dr. Croll's famous explanation of geological changes of climate, which he elaborated in a series of papers in the " Philosophical Magazine," and afterwards stated more fully in his well-known works " Climate and Time " and " Climate and Cosmology." Not only is this theory widely held, but it would be difficult to find one supported by greater skill and ingenuity, nor are there many works better worth reading and more inspiring than the two just named. I wish to say this the more emphatically because I cannot accept their conclusions.

There is nothing hesitating or apologetic about Dr. Croll's

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[6] Climate and Time, 88–89.

attitude. "I have studiously avoided," he says, "introducing anything of a hypothetical character. All the conclusions are based either on known facts or admitted physical principles. In short, the aim of the work is to prove that similar changes of climate follow, as a necessary effect, from admitted physical agencies, and that these changes, in as far as the past climatic condition of the globe is concerned, fully meet the demand of the geologist." [7]

It is not quite easy to concentrate the essential elements of Dr. Croll's argument, since his work contains several long parenthetical chapters discussing side issues. I will try to state his case as much as possible in his own words before I proceed to criticize it.

After expressing his agreement with those astronomers who cannot connect a glacial period with any change in the relations of the earth and sun, Dr. Croll goes on to say:— "The important fact, however, was overlooked that although the glacial epoch would not result *directly* from an increase of eccentricity, it might nevertheless do so *indirectly*. Although an increase of eccentricity could have no direct tendency to lower the temperature and cover our country with ice, yet it might bring into operation physical agents which would produce this effect." He then goes on to say that such a change in the eccentricity "would bring into operation a host of physical agencies, the combined effect of which would be to lower to a great extent the temperature of the hemisphere whose winters occur in aphelion, and to raise to nearly as great an extent the temperature of the opposite hemisphere, whose winters occur of course in perihelion." [8]

"With the eccentricity at its superior limit, and the winter solstice in aphelion, the length of the winters would exceed that of the summers by no fewer than thirty-six days. The lowering of the temperature, and the lengthening of the winter, would both tend to the same effect, viz., to increase the amount of snow accumulated during the winter, for, other things being equal, the larger the snow-accumulating period, the greater the accumulation."

[7] Preface, v. vi.   [8] Climate and Time, 15.
[9] Climate and Time, 58.

He then goes on to say, "While it is quite true that, however we alter the eccentricity, the increase of the sun's distance and the lengthening of the winter are compensatory, this is not so in regard to the snow accumulated. The consequence of this state of things would be that at the beginning of the short summer the ground would be covered with the winter's accumulation of snow." [1]

This postulating of a largely increased snowfall during the winters in question is taken for granted, and ignores certain very palpable facts. Thus, as Newcomb says :—"During the long cold winter the evaporation must be lessened, and hence the amount of precipitation also, unless warm and moist air is brought from the warmer regions of the globe. In this case the latent heat set free by precipitation, as well as the heat of the air itself, would mitigate the winter temperature." [2] Ice and snow cannot be formed without the liberation of a great deal of heat which is absorbed by the atmosphere and goes to mitigate the climate, and Dr. Croll has himself urged over and over again, with Tyndall, that it is rather heat than cold we need to explain a glacial period, heat to evaporate enough moisture and cause enough vapour. If the winter became excessively cold in the temperate zone, it is probable that the air in higher latitudes would be drained of all its moisture, and that virtually no snow at all would fall in very high latitudes. It would be the case of the Upper Andes and the Tibetan plateau over again, only that, instead of a zone of mountains to intercept and condense the damp in the air before it reaches its further destination, there would be a zone of cold in low latitudes to do so. I cannot see that Mr. Croll has at all shown that his colder winters would necessarily increase the total snowfall. His conclusion is reached by piling hypothesis on hypothesis, none of which are verified.

Having postulated an increased snowfall, he proceeds to argue that this very snowfall would act as a refrigerator. "The influence of snow and ice," he says, "*as a permanent source of cold* is one of the main factors of my theory." [3] Again he says, "For the last twenty years I have been

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[1] Id.   [2] Id. 271.   [3] Discussions on Climatology, 84.

maintaining that when a country is covered with ice it becomes a permanent source of cold, and however much heat may be received from the sun, the temperature of the surface can never be raised above the freezing-point while the ice remains ; and, again, that such an ice covering tends to its own preservation, because it chills the air and increases the snowfall. . . . In fact, had it not been for some such conservative power in the ice, a glacial epoch resulting from the causes which I have been advocating would not have been possible." [4]

Dr. Croll then goes on to argue as if this snow, which he claims would accumulate during his postulated long cold winter, would largely resist the summer heat and remain as a refrigerator to lower the summer temperature, and he specifies three ways in which it would do so. To these I will revert, and will at present limit myself to the problem as here stated. Now, granting that in spite of the above considerations, during long cold winters more snow would fall than in short ones, have we any warrant for supposing that the succeeding fiercely hot summers would not clear away the whole of the snow, and do a good deal more ?

The problem, as Mr. Hill says, is not to explain how there might be a continuously increasing mass of snow and ice, if we postulate the existence in the first instance of the necessary refrigerator. It is this postulate which has to be explained, and to this Dr. Croll devotes not a word. No doubt he refers to four causes which in his view might operate to accumulate snow and ice, if we could only begin with a large capital of cold stored up in ice and snow ; but, as Mr. Hill very properly says, " to set them at work, an accumulation of snow must have commenced, and since so long as no accumulation can begin they cannot operate, at least to any greater degree than at present, any discussion of this operation is superfluous." It is this first step which has to be explained, and which Dr. Croll turns the flank of, but does not face. To again quote Mr. Hill : " I always feel bewildered at his beginning with the winter. Has not summer as much right to be considered ? Why not say that in the intense heat of summer more polar ice will be melted than now ? [5] ' Yes,' says Dr. Croll, ' but

[4] Id. 124–5.
[5] It must be remembered that if the winters would be colder the

the winter is longer.'　But, I must rejoin, what have ᵒᵒ ort and long to do with the question?　The quantities of h t are unaltered, he admits.　Quantities of heat, not lengths of ᵗ ᵗ ᵗᵉ, are what we ought to look at.　To melt a mass of ice, a definite quantity of heat must be supplied; to form the same mass an equal quantity must be subtracted.　The amount of heat received annually will not be less than at present; no reason is given why the amount given off should increase.　If snow is to begin to accumulate, more snow must be formed than is melted; then either more heat must be lost annually, or else less received than is now the case.　But how will increased eccentricity be able to bring about either of these results?" [6]

This seems to me most conclusive, and the *a priori* position, thus stated is amply supported wherever we can apply a test.

Thus, the six feet of snow which fall on the Siberian tundras in the very prolonged winters that prevail there, which are much more severe both in duration and intensity than anything we can postulate as having been possible in a maritime climate like that of Western Europe, disappear in a few days, so that none remains at all, and the tundra is rapidly carpeted with flowers.　The same is true of the islands in the polar sea visited by Parry, except where the ground is very high.　Lieutenant Payer, of the Austrian Polar Expedition, found that during the short arctic summer of the highest latitudes the ice fields diminished four feet in thickness under the influence of sun and wind.　To replace this would require a precipitation of snow equivalent to about forty-five inches of rain, an amount which rarely occurs in lowlands out of the tropics. [7]　Alexander Agassiz, when living on the shores of Lake Superior, describes the thermometer as being at 5 below zero for four months in the year, and says that the average annual snowfall of fifteen years was seventy-two feet.　Yet the snow never lay more than six feet thick on the ground, and disappeared completely in the summer, the snow being chiefly got rid of by evaporation, like camphor. [8]

summers would be correspondingly hotter in times of great eccentricity.

[6] Geol. Mag. 1880, 12.

[7] Island Life, 135.　　　　[8] Lyell's Principles, i. 290.

Croll seems to argue as if the only element to be considered is the direct action of the sun upon the ice in summer and the action of the ice upon a stationary atmosphere; but this altogether overlooks by far the most important element in the whole problem. A very large portion of the snow which is melted in high latitudes is not melted directly by the sun, but indirectly through the medium of the warm winds which come from the south, and which, if not warm to start with, become warm very quickly by the condensation of the vapour they bring with them. Everyone knows the effect of the Föhn, the so-called glacier-eater. The action of the Föhn upon the Alpine snows is precisely paralleled by the action of the so-called Greenland Föhn in Greenland, and by the warm winds which eat up the snow in all northern latitudes, and which cannot have their action interfered with by the causes to which Croll attributes so much influence. The more we increase the contrast in temperature between high latitudes and low ones, the greater and more wider-spread will be the atmospheric circulation, and consequently the amount of melting which will ensue.

Wallace says, speaking of the Siberian snow, " it is not sensibly diminished by the powerful sun so long as northerly winds keep the air below the freezing-point and occasional snow-storms occur. But early in June the wind usually changes to southerly, probably the south-western anti-trades overcoming the northern inflow; and under its influence the snow all disappears in a few days, and the great vegetable kingdom bursts into full luxuriance. This is very important, as showing the impotence of mere sun-heat to get rid of a thick mass of snow so long as the air remains cold, while currents of warm air are in the highest degree effective." [9]

Another factor which seems to have been overlooked by Croll, and to which Sir Charles Lyell calls attention, is that a covering of snow, extending over a large area and enduring for a long time, must have the effect of preventing loss by radiation, snow being a very bad conductor of heat. He calls attention to the fact observed in Canada and New England, that parts of a meadow which are laid bare in winter by

[9] Island Life, 135.

the wind having blown away its snow, are often frozen for a depth of two feet or more, so that when spring returns this portion of the surface remains brown and barren, while the rest of the field is green and clothed with a rapidly growing vegetation, a check having been given by the snow to the radiation of heat. Dr. Hooker found in like manner that, after the melting of the snow on the Himalayas, the warmth of the soil was far above the mean temperature of the region, owing to the same cause. "In this way," says Lyell, "there may be some compensation, the excess of heat absorbed by the land during a short but hot summer being less freely parted with in winter owing to the snow. The loss by radiation during a protracted winter is only one of many elements, as yet undetermined, which complicate the problem on which we are now speculating." [10] I hold, therefore, that Dr. Croll's theory fails entirely at its very initial steps. He must account for a reservoir of cold existing all through the very hot summers which he postulates, if he is to get a fulcrum for his lever to work against. This he not only fails to do, but, in my view, it is futile to try and do so, for it involves an appeal to phantasm instead of fact.

Let us, however, advance somewhat. Suppose Dr. Croll has secured his refrigerator, how does he propose to use it? He tells us that the presence of so much snow as he postulates would lower the summer temperature and prevent to a great extent the melting of the snow, and this in three ways. First, by means of direct radiation. "No matter what the intensity of the sun's rays may be, the temperature of snow and ice tends by direct radiation, to lower the temperature of all surrounding bodies to 32°. . . . The air is cooled by *contact* with the snow-covered ground, but is not heated by the radiation of the sun." [11] Here again, as Newcomb says, the result is inconsequent. Of the fact that this cooling of the air would itself be necessarily accompanied by a melting of the snow Dr. Croll seems to show no consciousness. Yet there is no way in which the air can be cooled except by parting with its heat to the snow. Elsewhere Croll argues that the prolonged

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[10] Lyell's Principles, 10th ed. i. 284-5.
[11] Climate and Time, 58, 59.

winter which he postulates would diminish the midwinter temperature of Great Britain, sometimes to an amount of 30°, a conclusion which Newcomb regards as entirely untrust-worthy, being founded on purely hypothetical laws with purely hypothetical data.[1]

But suppose we grant it; let us measure its effect. A simple calculation will show that the cooling of the air by some 36° Fahr. would only have the effect of melting twenty inches of snow, which is a most liberal allowance.

Newcomb says further, in reference to this point, that Croll seems to argue as if the solar heat absorbed by the atmosphere is entirely lost, so far as warming any region of the globe is concerned, and he urges that in the polar regions a greater percentage of the heat is absorbed by the atmosphere than at the equator. "From the care he takes to subtract this percentage from the amount of heat received by the polar regions, he seems to think that the heat thus absorbed is totally lost, and does not warm the atmosphere at all. But a moment's reflection must show that as all this absorption must occur within three or four miles of the earth's surface, and probably half of it within a single mile or two miles at most, while the arctic regions are more than 2000 miles in diameter, it makes no difference what portion of the heat is absorbed by the atmosphere. In the one case, the atmosphere is warmed directly by the absorption of heat, in the other, by contact with the earth; but the temperature of the region is substantially the same in either case."[2]

The ice lowers the temperature of surrounding bodies warmer than itself, not by radiating cold to them—cold is a mere negation—but by absorbing heat from them. The only sun-heat which reaches the ice and is lost is that which is reflected back into space. The rest either goes to melt the ice or to vaporize it.

So long as the ice remains unmelted, so long, no doubt, will the air in contact with it be at 32°, and so long will the temperature be cold; but I cannot see how this will prevent or interfere with the action of the sun's rays undoing in a

[1] Silliman's Journal, 1876, 271.
[2] Id., 269-271.

fiercely hot short summer what the absence of those rays has done in a long winter, namely, removing the ice, unless it is argued that much the larger proportion of those rays are reflected. If so, how are we to account for what takes place in the very short summers on the Siberian tundras and on the Parry islands?

Dr. Croll's second cause is that while a large number of the sun's rays are reflected into space, those that remain, instead of raising the temperature, disappear in the mechanical process of melting the ice. The latent heat of ice being about 142 Fahr., the melting of every pound of ice requires a quantity of heat sufficient to raise a pound of water 142°, and this is completely lost, so far as temperature is concerned. This quantity of heat is consumed, not in raising the temperature of the ice, but in the mechanical work of tearing the molecules separate against the forces of cohesion binding them together into the solid form. No matter what the intensity of the sun's heat may be, the surface of the ground will remain permanently at 32° so long as the snow and ice continue unmelted.[3]

First, in regard to the reflecting power of the snow, Professor Newcomb seems to me to be equally unanswerable. Referring to the notion, he says: " How great the reflecting and transmitting power of the snow and ice must be, to keep the snow unmelted all summer, may be inferred from the fact that during the postulated perihelion summer the amount of heat received from the sun by every part of the northern hemisphere would suffice to melt four to six inches of ice per day, over its entire surface, that is, it would suffice to melt the whole probable accumulation in three or four days. The reader can easily make a computation of the incredible reflecting power of the snow, and of the unexampled transparency of the air required to keep the snow unmelted for three or four months." [4]

It must be remembered what the short summers which accompanied the portentous winters of Mr. Croll really were. According to his own method of estimating solar temperature, the midsummer heat of the northern hemisphere, due to solar

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[3] Id. 60.   [4] Id. 271, 272.

radiation, was from 40° to 50° higher than it is now. His layer of snow must therefore have resisted, not merely our present heats, but temperatures ranging from 100° to 150° Fahrenheit.[5]

Secondly, as to the argument that a large portion of the heat is absorbed in melting the ice and not in raising the temperature.

Here again I cannot follow the reasoning, and I cannot see how Mr. Searles Wood's reply is to be got over.[6] It is quite true that in order to melt a pound of ice 142° of heat would be expended, before the temperature of the air would be raised at all. That is no doubt so ; but if so much of the summer heat is absorbed in melting the ice, we must remember that a precisely equal amount is given out in the winter when the ice is formed. The one process exactly corresponds to the other, and whatever amount of the sun's rays is taken from the air, etc. in summer to melt the ice, precisely the same amount is given back to the air, etc. in the winter when the ice is made ; so that there is absolute compensation, and I therefore cannot see how this cause can be an efficient one. A reviewer of Mr. Croll's book in *Nature* makes a similar objection. He says : " Some of his arguments on this point are rather circular, for whatever amount of heat is rendered latent in the melting of ice, as much will be supplied to radiation in the freezing ; and no *increase* of ice could arise from this."[7]

Mr. Croll's third cause is that the snow and ice would lower the temperature by chilling the air, and condense the vapour into thick fogs, which would effectually prevent the sun's rays from reaching the earth, and the snow in consequence would remain unmelted during the entire summer.

To this Newcomb replies : " He says nothing about the latent heat set free by the condensation, nor does he say where the heat goes to which the air must lose in order to be chilled." Professor Newcomb goes on to complain of the difficulty of arguing with an opponent who in one breath maintains that the transparency of the air is such that the rays reflected from the snow pass freely into space, and in the

[5] Id. 273.　　　[6] See Geol. Mag. 1876, 389.
[7] Nature, xii. 142.

next breath that thick fogs effectually prevent the rays ever reaching the snow at all. . . . If the snow, air, fog or whatever throws back the rays of the sun into space is so excellent a reflector of heat, it is a correspondingly poor radiator, and the same fog which will not be dissipated by the summer heat will not be affected by the winter's cold, and will therefore serve as a screen to prevent the radiation of heat from the earth during the winter."[8]

Mr. Searles V. Wood has argued in a similar strain. If it be true that the fogs intercept the sun's rays and prevent them from directly reaching the ice, is it not equally true that in becoming fogs the vapours " give out in condensing an amount of heat equal to that absorbed in their creation, and thus all the heat intercepted by fogs would be eventually restored to the atmosphere of the icy region ?"[9] It is forgotten that every snow-storm adds enormously to the heat in the air, and *pro tanto* moderates the climate which would otherwise be so severe ; and we cannot at the same time attribute an intercepting power to fogs, without also remembering that every degree of heat they intercept is kept here instead of radiating or being reflected into space.

At every point where we can come face to face with his subtle arguments, Croll seems to reach his conclusions by ignoring great classes of facts which it is necessary to remember if we are to make a substantial advance in this most intricate subject, and at every point his position seems honeycombed with fallacies. Nor am I quite sure that he did not himself feel that the ground traversed in the arguments already quoted was uncertain, for he more than once puts them aside as quite subsidiary, and condenses his main arguments upon another cause, which has not been, it seems to me, sufficiently examined, and to which I must devote a longer space. Referring to the various causes of the Glacial epoch, he says : " By far the most important of all these agencies, and the one which mainly brought 'about the Glacial epoch, is the *Deflection* of ocean currents."[1] Elsewhere he refers to the deflection of ocean currents as a far more potent agency than any of those he had noticed.[2]

[8] Op. cit. 272.   [9] Op. cit. 390.
[1] Climate and Time, 14.   [2] Id. 68.

In his essays on Climate, etc., he thus states this part of his case. " When," he says," the eccentricity of the earth's orbit is at a high value, and the northern winter solstice is in perihelion, agencies are brought into operation which make the south-east trade winds stronger than the north-east, and compel them to blow over upon the northern hemisphere as far, probably, as the Tropic of Cancer. The result is that all the great equatorial currents of the ocean are impelled into the northern hemisphere, which thus, in consequence of the immense accumulation of warm water, has its temperature raised, and the snow and ice to a great extent must then disappear from the arctic regions. When the precession of the equinoxes brings round the winter solstice to aphelion, the condition of things in the two hemispheres is reversed, and the north-east trades then blow over upon the southern hemisphere, carrying the great equatorial currents along with them. The warm water being thus wholly withdrawn from the northern hemisphere, its temperature sinks enormously, and snow and ice begin to accumulate in temperate regions. The amount of precipitation in the form of snow in temperate regions is at the same time enormously increased by the excess of the evaporation in low latitudes resulting from the nearness of the sun in perihelion during summer." [3]

This is the main part of the glacial theory as taught by Dr. Croll, and it involves the conclusion that there has been throughout geological time an alternate glaciation of each hemisphere ultimately caused by changes in the eccentricity of the earth's orbit, and directly engendered by the greater amount of warm water forced into each hemisphere by the alternately greater potency of the trade winds north and south of the equator. This being the theory, let us now examine it with the care which its wide acceptance demands.

I may at once say, that at least two of Dr. Croll's most important postulates seem incontrovertible.

His prime postulate is that the chief factor in determining the distribution of heat over the world is the direction of ocean currents. This he emphasizes in many ways. *Inter*

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[3] Climate and Cosmology, 15, 16.

alia, he says, " It will be seen that to such an extent is the temperature of the equatorial regions lowered, and that of high temperate and polar regions raised, by means of ocean currents, that were they to cease, and each latitude to depend solely on the heat received directly from the sun, only a very small portion of the globe would be habitable by the present race of beings. This being the case, it becomes obvious to what an extent the deflection of ocean currents must affect temperature.[4] With this general postulate I have no quarrel. It seems to me to be sound, and to be supported by abundant evidence. So, it seems to me, is the following more concrete corollary which he draws from it.

Applying himself especially to the Gulf Stream, he calculates that the quantity of water it conveys is probably equal to that of a stream fifty miles broad and 1000 feet deep, flowing at the rate of four miles an hour, and that its mean temperature on leaving the Gulf of Mexico is not less than 65°. This is one-half only of the volume of the stream as calculated by Maury, Herschell, and others. Taking its speed at only two miles an hour, and the average temperature of the return stream at 40°, showing a loss of 25° of heat, Dr. Croll argues that the quantity of heat which is transferred daily by the Gulf Stream is 77,479,650,000,000,000,000 foot-pounds, and that the quantity conveyed by it in one year is equal to the heat which falls on an average on 3,436,900 square miles of the arctic regions. As the frigid zone or arctic regions contain 8,130,000 square miles, there is therefore nearly one half as much heat transferred from tropical regions by the Gulf Stream as is received from the sun by the entire arctic world, the proportion being nearly as 2 to 5,[5] and this without taking into calculation the greater amount of heat absorbed by the air in the arctic regions, in consequence of the obliquity of the sun's rays, which in the article " Climate " in the E. B. is calculated to be 75 per cent. Comparing the quantity of heat conveyed by the Gulf Stream with that conveyed by aerial currents, he argues that it would require a current of air 31,234 times the volume of the Gulf Stream at the same temperature to convey the same heat, and he con-

[4] Climate and Time, 15.   [5] Id. 24-27.

cludes that the quantity of heat conveyed by the Gulf Stream alone from the tropical to the temperate and arctic regions is probably greater than by all the aerial currents which flow from the equator. And he argues with force that since the winds flowing from the equator are the anti-trade winds, which come from an altitude above the snow-line, and therefore below 32° in temperature, through which they have to blow for some days, they cannot bring much, if any, heat from thence.[6] It is generally admitted that the Gulf Stream causes the mean annual temperature of our island to be 12° above the normal temperature due to the latitude, but Dr. Croll shows that this underrates the total effect of the Gulf Stream in these latitudes, since the mean referred to is itself raised by other tropical currents in the Pacific. He also shows that the very great flexure of the isothermic lines in the North-east Atlantic can be due to nothing else but the Gulf Stream, and he adds : " On no point of the earth's surface does the mean annual temperature rise so high above the normal as in the Northern Atlantic, just at the arctic circle, at a spot believed to be in the middle of the Gulf Stream. This place is no less than 22°·5 above the normal, while in the North Pacific the temperature nowhere rises more than 9° above the normal." [7]

With this reasoning I have no quarrel whatever ; and if such be the effects of the Gulf Stream, we may well say, with Dr. Croll, how enormous must be the quantity of heat conveyed from the equatorial regions by all the ocean currents together !

I have given a *résumé* of Dr. Croll's arguments on this question, because I believe them to be substantially sound and incontrovertible. The postulate, then, that the climate of the earth is largely the result of the distribution of ocean currents seems to me reasonable. This in effect means that the climate is largely due to the effect of the great equatorial current, which is the main liquid reservoir of heat, and of which the Gulf Stream in the Atlantic, the Karo Siwo in the Pacific, etc., etc., are mere prolongations and branches. So far I raise no issues with Dr. Croll.

[6] Id. 27-28.   [7] Id. 46.

His next postulate is that the ocean currents are due to the blowing of the winds. Here again I believe him to be substantially right.

After criticizing the theories which attribute the flow of ocean currents to the rotation of the earth and to gravity, and, in my view, showing them to be impossible, Dr. Croll goes on to argue that they are the result of the pressure and traction of winds, a view in which he was forestalled in a valuable paper published as far back as 1851, by Alderman Thomas Hopkins, in the *Memoirs* of the Lit. and Phil. Society of Manchester, which he would no doubt have quoted if he had known of it, for he was a model of fairness as well as lucidity. Dr. Croll has elaborated many arguments to show that winds are competent to create ocean currents, and that the observed facts can be explained, and explained most satisfactorily, by their agency, and there can be small doubt that here again he is substantially right. But while conceding that the winds are the main cause of ocean currents, I cannot go so far as to deny the existence of at least one minor and effective cause.

The great tropical current, which for our present purpose is the most important, seems to have at least one other impulse to which Dr. Croll has not given due weight. As Réclus puts the case, "Every difference of level which is produced on the liquid surface in consequence of prolonged winds, *heavy rains, or very active evaporation,* causes as a necessary result, the formation of a current, for the water, whether salt or fresh, ever seeks its level, and incessantly flows from the more elevated places towards the depressions. . . . The equatorial basin, incessantly heated by the solar rays, loses a great quantity of water, which is transformed into vapour, and rises into the higher strata of the atmosphere, to be condensed into clouds. Admitting that the annual evaporation is about fourteen feet, which is probably below the reality, the quantity of fluid raised from the Atlantic in the tropical zone would be nearly 120 billions of cubic yards, and would consequently represent the same value as a cubic mass of water nearly thirty miles in extent. It is true that a considerable part of this vapour, the half perhaps, falls as rain into the seas situated beyond the tropics, and

over the neighbouring continents. Near the equator, there-
fore, much more water is drawn from the ocean by evapora-
tion than is restored to it by the clouds, and, in conse-
quence, an immense void is formed which can only be filled
by the waters from the polar basins, where the contributions
of snow, rain, and ice exceed the loss in vapour. This
superabundant mass of fluid continually flows towards the
basin of the torid zone, and forms the two great currents
which meet one another from the opposite poles in the
Atlantic and the Pacific, incessantly describing a regular orbit
like the celestial bodies. But the excess of evaporation which
occurs in tropical waters is not the only reason of this great
movement of the polar seas towards the torrid zone. The
trade winds, attracted by the force of equatorial heat, blow
incessantly in the same direction, and always driving the
waves before them, thus accelerate the march of the oceanic
current."[8]

With this argument, except the part which brings the
intrusive water from the poles, I very largely agree; and it
seems to me that, if there were no winds at all, but evapora-
tion were to go on as it does now, it would inevitably cause
a certain circulation of water between the torrid and the
colder zones, and I cannot, therefore, attribute all this move-
ment to the wind, as Dr. Croll seems disposed to do. This,
however, is a minor issue, and it seems clear that the winds
are, as he makes them, the really efficient factors in creating
ocean currents.

So far I have no dispute with Dr. Croll. I would remark
that the discussion of these postulates, about which I raise no
question, forms a substantial part of " Climate and Time,"
occupying at least 130 pages of it, and that his clear and
incisive criticism of Maury, Carpenter, and others, produces
an impression in casually reading the book that, in destroy-
ing their arguments, he is establishing his main position,
which is not so, these polemics being really side issues,
and filling only a subsidiary part in the main argument.

Having granted two of Dr. Croll's postulates, namely, that
climate is largely dependent on the distribution of ocean

[8] Réclus, The Ocean, 60, 61.

currents, and that these are chiefly dependent on the winds,
let us now come to closer quarters, and see whether the
demands he makes upon the aerial currents are justifiable.

Meteorology is a difficult inquiry, and full of pitfalls. If
we are to tread its quagmires with any confidence, we must
do so in very elementary fashion. " If," says Mr. Buchan,
" atmospheric pressure were equal in all parts of the earth,
we should have the physical conditions of a stagnant atmo-
sphere. Such, however, is not the case. Let there be pro-
duced a concentration of aqueous vapour over a particular
region, or let one region show a higher temperature than
what prevails around it, then from the different densities and
different pressures thereby produced the equilibrium of the
atmosphere is destroyed, and, as might be expected from the
laws of aerial fluids, movements of the air or winds set in to
restore the equilibrium." [9] This is incontestible. So is the
following :—" All winds may be regarded as caused directly
by differences of atmospheric pressure, just as the flow of
rivers is caused by differences of level, the motion of the air
and the motion of the water being both referable to gravita-
tion. The wind blows from a region of higher towards a
region of lower pressure—in other words, from where there is
a surplus to where there is a deficiency of air." [1] The next
argument is equally sound. " So far as is known, differences
of atmospheric pressure, and consequently all winds, originate
in changes occurring either in the temperature or the
humidity of the air over restricted regions. Thus, if two
regions contiguous to each other come to be of unequal
temperature, the air of the warmer region, being specifically
lighter, will ascend, and the heavier air of the colder regions
will flow in below to take its place. Of this class of winds
the sea and land breezes are the best examples. Again, if
the air of one region comes to be more highly charged with
aqueous vapour than the air of surrounding regions, the air
of the more humid region, being lighter, will ascend, while
the heavier air of the drier regions will flow in below, and
take its place. Some part of the vapour will be condensed
into cloud or rain as it ascends, heat will be thereby dis-
engaged, and the equilibrium still further disturbed." [2]

      [9] E. B. xvi. 143.   [1] Id.   Id.

The paragraphs I have just quoted state clearly some of the elementary postulates of general pneumatics. Let us now apply them to the earth. As is familiar to everybody, the sun's rays exercise their most potent influence in the neighbourhood of the equator, which may be looked upon as a terrestrial furnace, and their least at the two poles, which may be considered as two refrigerators.

It follows that about the equator the air is not only being heated and expanded, but, since heat is the great evaporator, it is also being charged with moisture more rapidly than it is elsewhere, and that if the earth were homogeneous, and entirely covered with water, there would be a progressive lightening of the air going on from its densest and least humid portions at the poles to its most expanded and most humid at the equator. That is to say, there would be a continuously diminishing atmospheric pressure as we neared the equator from the poles. The result would be that a column of damp and expanded, and therefore light, air would rise up continuously round the equator. In order to restore equilibrium, the void thus created would be filled by an indraft from the colder regions on either side, the ascending column would presently spread out in both directions, north and south, and thus a circulation would begin causing near the ground a more or less north wind in the northern hemisphere, and a south wind in the southern, to blow towards the equatorial region, while above these two winds would be two other currents flowing in the opposite direction. If the earth were a cylinder and not a sphere, and there were no cold of space to invite radiation and cool the currents of air as they rise from the ground, and if its surface were homogeneous, and covered all over either with water or dry land, there would probably be a perfect and continuous circulation of winds between the equator and the poles—the postulated foci of least and greatest barometical pressure. The three factors just named, however, introduce a very different condition of things, and limit the area over which these winds can blow very materially.

In the first place, let us turn to the shape of the earth. Inasmuch as the earth is a sphere, with its furnace round the equator and its refrigerator at each pole, each hemisphere

becomes covered with a funnel of air, with its terminus at the pole *closed,* and thus it comes about that the polar area cannot find room for all the air which is heated at the equator.

Professor Tyndall puts the case clearly :—" On this terrestial globe," he says, " I trace with my hand two meridians. At the equator of the globe they are a foot apart, which would correspond to about 1000 miles on the earth's surface. But these meridians, as they proceed northwards, gradually approach each other, and meet at the north pole. It is manifest that the air which rises between these meridians, in the equatorial regions, must, if it went direct to the pole, squeeze itself into an ever-narrowing bed, and the space around the poles is unable to embrace the air from the equator." [3] This is a most important fact, which has been overlooked in some speculations on meteorology, and it interferes completely with those theories which look so symmetrical, and which postulate an exchange of air between the equator and the poles, an exchange which cannot physically take place on a considerable scale on a spherical body like the earth. Another interference is caused by the cold of space, which has the effect of cooling the upper air which has risen in the equatorial region, and thus making it denser after it has travelled some distance, until it becomes heavier than the subjacent air, and must sink by the force of gravity long before it reaches the two poles. The presence of great continental areas separating the great oceans, where heat is more quickly absorbed and more quickly radiated than over water, must also interfere somewhat with the continuity of the circulation. We may, however, neglect this at present, and consider what must be the result of the operation of the other two causes. There can be little or no doubt that the result must be to greatly shorten the journey of the equatorial air in its circuit, and to greatly lessen the course of the winds, which flow towards the equator, and this is exactly what we find to be the case. The trade winds do not blow from the polar or even from the temperate zones, but they blow from latitudes even nearer the equator, their limits being bounded

Heat as a Mode of Motion, 168-9.

towards the north and south by the so-called calms of Cancer and Capricorn respectively.

As we have seen, the column of equatorial air, on reaching a certain height, spreads out and begins to flow to the north-east and south-east, as the anti-trade winds. As Mr. Scott says, this overflow takes place at a great height, for the level of the counter current has never been reached by the ascent of any of the peaks in the equatorial Andes; but that the air does flow away in the upper strata is abundantly proved by the fact that clouds are often noticed moving in a direction contrary to that of the trade wind below. A proof even more striking is afforded by the fall of ashes from the volcanoes in Central America in the Spanish Main, and from those in the Dutch East Indies in the Western Pacific. An instance of the former occurrence was recorded in January, 1835, when ashes from Coseguina, on the Bay of Fonseca, were borne in four days to Kingston in Jamaica, in the teeth of the trade wind. The latter phenomenon took place in 1815, when the ashes from Tomboro, in Sumbawa, were carried to Amboyna, a distance of 800 miles, athwart the direction of the south-east trade.

" When we reach the outer edge of the trade wind zone we find the counter current gradually descending to the sea level. On the Peak of Teneriffe (lat. 28° N.), and on Mauna Loa, in the Sandwich Islands (lat. 20° N.), there are constant south-west winds at the summit. . . . When these upper currents reach the earth, about the latitude of the tropics of Cancer and Capricorn, a great portion of the air which goes to form them is caught in the under current and drawn back to the equator; but the remainder flows on; and outside the parallels named, in both hemispheres we can no longer trace the persistence of upper and lower currents at all seasons, but the predominant direction of the air motion is from the equator towards the pole; and this appears to increase in steadiness and force with the height above the ground." [4]

This clearly proves that the circulation of the trades and anti-trades is limited to the region between the equatorial calms on the one hand, and the calms about the two tropics

Elementary Meteorology, by R. H. Scott, 244-46.

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of Cancer and Capricorn respectively, as we should in fact expect from an examination of the tables of barometric pressure. The mean pressure in the equatorial belt is only about 29·84 inches, while between the 30th and 35th parallels it reaches its maximum of 30 to 30·08 inches; and as we have seen, and as has been clearly proved, the lower winds blow from situations of high pressure to those of low pressure, while the return upper currents go the reverse way.

It is most important in the discussion we are engaged in to remember this limitation in area of the trade winds, for it is the trade winds alone with which we have virtually to deal, since they give the equatorial current its principal impulse.

With the winds north and south of the two tropics we have nothing to do in this discussion. They are much less constant than the trades, their course being subject to many influences, but in the main it is probable that there is a general circulation between the temperate and the arctic regions. This circulation, however, belongs to an entirely different system, and does not, so far as we can see, interfere with the trades and the anti-trades, which form a circuit of their own. Dr. Croll has largely ignored this fact. He argues as if the strength of the respective trade winds is dependent on the fact that the difference in temperature between the equator and the north pole is not the same, but greater than the difference between the respective temperatures of the equator and the south pole. *Inter alia,* he says: "As the existence of the winds depends mainly on the difference of temperature *between equatorial and polar regions,* any cause which will greatly affect this difference of temperature will likewise greatly affect the winds." He argues that an increase or decrease in the difference of temperature between equatorial and polar regions would produce an increase or decrease in the strength of the winds, and consequently in the strength of the currents, and that a *difference in the difference of* temperature between the equator and the two poles would affect the distribution of aerial and consequently of ocean currents, and would in turn produce a corresponding increase in the difference of temperature

between the two hemispheres. This change, again, would tend to strengthen one trade wind and to weaken the other, etc., etc.

I cannot agree with this conclusion, which is at issue both with *a priori* and with empirical tests.

The chief condenser and refrigerator, where the warm damp air of the torrid zone is deprived of its heat and moisture, is not the area at each pole, but the upper region of the atmosphere.

As we have already shown, the warm current which rises at the equator rises at least higher than the peaks of the Andes, that is far above the snow-line, and into a region which but for this current would be perpetually below 32° in temperature, and it remains in the cold region during the greater part of its journey to the two tropics.

It does not matter, therefore, how cold the poles are made, they cannot drag or pull this equatorial current of air towards them. It has much closer at hand, namely, in the regions of space, a much colder refrigerator than either pole, which must inevitably cool it down and deprive it of moisture, and therefore force it to descend and come to the ground long before it can reach the poles.

The polar cold, therefore, cannot affect the circulation of the trade winds. The hotter the equator, the hotter and more charged with vapour will the air be that rises there. That is true, but this will only make it more buoyant and rise higher to a colder stratum, and thus the greater heat of the furnace will be compensated by the greater cold of the stratum to which the air will rise, that is, of the refrigerator. It is well known that the equatorial current discharges its great mass of moisture in the shape of very heavy rain (that is to say, has its moisture condensed) before it reaches the two tropics.

This makes it clear that the trade winds belong to a circulation of air outside of polar influence altogether, and are not affected, as Dr. Croll argues, by a greater or less temperature at the poles.

Let us now advance another step, limiting ourselves strictly to the trade winds. If the earth were still, the trades would move directly towards the parallel of greatest heat, while the

anti-trades would move directly away from it, and their motion would be at right angles to the equator. This direction, however, is interfered with by the rotation of the earth, for although this rotation will not induce a movement in anything placed upon the earth, it will, as we have seen, deflect the direction of anything already in motion.

" In virtue of the rotation of the earth," says Mr. Buchan, " objects on its surface at the equator are carried round towards the east at the rate of about seventeen miles a minute on receding from the equator. This rate of velocity is being continually diminished, so that at 60° N. lat. it is only about 8½ miles a minute, and at the poles nothing. From this it follows that a wind blowing along the earth's surface, in the direction of the equator, is constantly arriving at places which have a greater eastward velocity than itself. As the wind thus lags behind, these places come up, as it were, against it, the result being an east wind. Since, therefore, the wind north of the equator is under the influence of two forces—one, the low pressure near the equator, drawing it southwards, and the other, the rotation of the earth, deflecting it eastwards—it will by the law of the composition of forces take an intermediate direction, and blow from north-east. For the same reason, south of the equator the south is deflected into a south-east wind." [5]

While the rotation of the earth gives the two trade winds a certain westerly motion, it for the same reason gives the anti-trades an easterly movement, since in this case the winds are moving from a latitude where the earth has a great, to one where it has a small, rotary movement. That this is so we can tell by the direction taken by the volcanic ashes carried by the anti-trades, to which we have already referred. The result, then, is that the trades and anti-trades, instead of blowing directly to and from the equator, blow at a certain angle to it.

Between the north-east and south-east trades is the well-known belt of equatorial calms. These calms Dr. Croll, following other writers, attributes to the conflict of the two sets of winds, which, according to him, create a neutral belt between them. To this view I cannot at all assent.

[5] E. B. xvi. 103.

If the effect of heat and the addition of moisture upon air were to leave its density and specific gravity unaltered, and merely to induce it to move laterally, and thus form a wind, the argument might hold good ; but the immediate effect of adding heat and moisture to air is to make it lighter, and also to expand it, and it must begin to move in the direction of least resistance, that is, to ascend. As the cold air from the pole gradually moves towards the equator, it gets more and more heated, and more and more full of moisture, and more and more expanded and buoyant. On the contrary, that which proceeds from the equator towards the poles gets more and more contracted and heavier. The former current therefore acquires, in addition to its horizontal movement, first a *tendency* to rise, and then an actually upward movement, and the latter a corresponding *tendency* to sink, and eventually a downward movement; and when the focus of heat and cold is reached this upward and downward movement respectively becomes dominant, and the currents of air do not press against each other, but rise up almost vertically, and lose their lateral motion. It does not seem to me that there can be any pressing of one set of winds against the other, and both the north and south trades, on reaching their furnace, must move upwards, and not laterally. The region of calms merely marks the place where the vertical movement upwards and downwards takes place.

It is clear that the winds could not under any circumstances meet each other face to face, since, as we have seen, the motion of the earth gives them a very decided motion from east to west. If they collided at all, therefore, instead of neutralizing each other, they would acquire a more direct course westwards, as the corresponding currents of water do; and instead of a region of calms in the tropics we should have a region of more or less violent east winds along that zone, of which I know of no adequate evidence. In every way the question is examined, it seems to me to be impossible to attribute the equatorial calms to the neutralizing tendency of a conflict of winds, as Dr. Croll urges, and this is a most important factor in his argument.

Let us proceed, however. It is perfectly true, and an

elementary fact in physical geography, that the two trade winds do not always blow between the same parallels, and that the region of intervening equatorial calms is not stationary, but that the whole move together, to and fro, north and south, as the sun moves from one equinox to the other. This movement, Dr. Croll seems to argue, is due to the greater strength of the north trade at one time of the year than at another, enabling it to force back the other trade wind and the calms several degrees. As we have seen, there is no evidence of any pushing at all, and such a postulate is contrary to all we know.

The movement of the trades to and fro is in fact due to another and a very simple cause. The fact is that the circulation of the two great constant winds has nothing to do with the earth's *equator of form* at all. Their circulation is induced by the heating effect of the sun, and it is to where the furnace burns the hottest, that is to say, to the latitude of greatest mean heat, that these winds, by the laws of pneumatics, must tend. Now the latitude of greatest mean heat does not coincide with the equator, and wherever it happens to be, *there* must be the focus of the calms, and there the medial line between the trades. It is the situation of the furnace of greatest heat for the time being, and not the relative strength of the trades, that fixes the situation of the combined belt of trade winds and equatorial calms. This is not mere conjecture, but it agrees with observed facts as well as with theory, and it must be true whatever eccentricity we may postulate for the earth's orbit.

I cannot, therefore, at all agree with Dr. Croll's remarks on the subject when he says : " It has been shown that an increase in the eccentricity of the earth's orbit tends to lower the temperature of the one hemisphere, and to raise the temperature of the other. The consequence of this is that the aerial currents of the hemisphere, in which the difference of temperature between the equatorial and the temperate and polar regions is greater, is stronger than those of the other." " This," he adds, " would be more especially so with the trade winds. The stronger trades would blow across the equator and drive the median line between them and the opposite trades into the other hemisphere. If the north-east trades,

for instance, were the stronger, the equatorial waters would be impelled more to the southern than to the northern hemisphere; and the warm water carried over in this manner to the southern hemisphere would tend to increase the difference of temperature between the two hemispheres. This change, again, would in turn tend to increase the north-east and to weaken the south-east trades, and would thus induce a still greater flow of equatorial waters into the southern hemisphere, a result which would still more increase the difference in temperature between the northern and southern hemisphere, and so on—the one cause reacting on the other, so as to increase its effects." [6]   Having stated this principle in general terms, Dr. Croll proceeds to test it by a critical example, and says :—" We have in the present state of things a striking example of the extent to which the median lines between the two trades may be shifted, and the position of the great equatorial currents of the ocean may be affected by a slight difference in the relative strength of the two aerial currents. The south-east trades are at present a little stronger than the north-east, and the consequence is that they blow across the equator into the northern hemisphere to a distance sometimes of 10° or 15° ; so that the mean position of the median line lies at least six or seven degrees north of the equator. And it is doubtless owing to the superior strength of the south-east trades that so much warm water crosses the equator from the South to the North Atlantic," etc., etc.[7]

This reasoning seems to me to be eminently fallacious. In the first place, I know of no adequate evidence for the conclusion that the south-east trades are now stronger than the north-east trades. It is quite true that the belt of calms which separates the two trades is situated very largely north of the equator. But this, I urge very emphatically, is due to an entirely different cause, namely, the fact that the equator of heat of the earth is situated to the north of the real equator. I will on this point quote the lucid notice of Reclus :—" The complete system of the winds," he says, " inclines towards the northern hemisphere, and it is to the

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[6] Climate and Time, 227-29.   [7] Id. 230-31.

north of the line that the girdle of the equatorial calms is in
all seasons developed. This phenomenon, which might seem
at first sight strange, results from the grouping of the greater
part of the continental lands in the northern hemisphere, and
from the difference of temperature, which must be, for our
part of the world, the result of this unequal distribution of
solid and liquid. It is also in the northern hemisphere that
we find the Desert of Sahara, that immense extent where
wooded tracts are relatively few, and where the reflection
from the burning sands and rocks vaporizes the clouds which
the atmospheric currents bring. The Sahara, and, in a less
degree, all the tropical countries of the northern hemisphere,
act as a great centre, towards which the aerial masses flow.
It results from the tables drawn up by Dove that the mean
temperature of the year is more elevated (92 Fahr.) towards
the 10th degree N. lat., than it is at the equator itself (91·5 F.),
while the mean balance is stronger towards the 20th degree
of lat. (94 Fahr.) than in any other region of the world."
Reclus adds: "The high temperature of the continents thus
forces the southern winds to encroach upon the northern
systems."[8] This northern position of the equator of heat is
not only the mean of its several movements, but we shall find,
as a matter of fact, that it never goes south of the equator
at all. "When the sun," says Reclus, "after the 21st of
September, crosses the equatorial line to tend towards the
tropic of Cancer, the centre of the trade winds, and conse-
quently of the band of calms, moves at the same time towards
the north; on the contrary, when the sun returns to the
tropic of Capricorn, the most heated zone of air is gradually
brought back to the south with the whole circulatory system
of the trade winds. At the end of March, the northern limit
of the equatorial calms of the Atlantic is found on an average
towards the 2nd degree of north latitude, while at the end
of September the same limit attains ordinarily the 13th or
14th degree. As to the southern limit, it oscillates in the
same ocean from one to four degrees of *north* latitude."[9]

Here, then, we have the real and efficient cause of the
movement to and fro of the trade winds and the equatorial

[8] Reclus, The Ocean ; translated by Woodward, i. 243-4.
[9] Id. 292-3.

calms, and so long as the parallel of greatest mean heat, that is to say, so long as the furnace which causes the circulation of the trades remains to the north of the equator, so long will the south-east trades blow across the equator, not because of their greater potency, but because they must travel thither to reach the furnace which makes them move at all. This seems to be a complete answer to the main contention of Dr. Croll, which he states as follows :—

". . . When the eccentricity of the earth's orbit attains a high value, the hemisphere whose winter solstice occurs in aphelion has its temperature lowered, while that of the opposite hemisphere is raised ; let us suppose the northern hemisphere to be the cold one, and the southern the warm one. The difference of temperature between the equator and the North Pole will then be greater than between the equator and the South Pole ; according, therefore, to theory, the trades of the northern hemisphere will be stronger than those of the southern, and will consequently blow across the equator to some distance on this southern hemisphere. This state of things will tend to deflect equatorial currents southwards, impelling the warm water of the equatorial regions more into the southern or warm hemisphere than into the northern or cold hemisphere. The tendency of all this will be to exaggerate the difference of temperature already existing between the two hemispheres." [1]

This position, as I have tried to show, fails at every point. The circulation of the trades does not depend on the difference between the temperature of the equator and the poles, for the warm equatorial winds and the cold polar winds each have a separate and distinct circuit of their own. There is no evidence that the south-east trades are stronger than the north-east, nor, if they were, that they could push back the region of equatorial calms, since, on reaching the parallel of greatest heat, they must move vertically and not laterally. On the other hand, the fact that they blow across the equator is accounted for by the situation of the parallel of greatest mean heat being to the north of that great circle, which necessarily causes them to blow across the equator, and,

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[1] Climate and Time, 13-17.

what is more important, would do so as long as that parallel remained north of the equator, however great the eccentricity of the earth.

What Dr. Croll has to prove in order to substantiate his position is, that during periods of greater eccentricity, when *our* winter took place with the world in aphelion, the equator of greatest mean heat was transferred to the other hemisphere, and that it has been subjected to an alternate movement north and south. That is to say, since the position of the equator of greatest mean heat is dependent upon the distribution of land and water, and the fact that in the torrid zone there is at present a larger proportion of land to water north of the equator than south of it, that this proportion was different in so-called glacial times, and that consequently there has been since those times a great revolution in the contour of the continents. Against such a conclusion the evidence of geology is very uniform, and it is in fact fully admitted by Dr. Croll himself, who, speaking of the so-called glacial epoch, says :—" Geologists almost all agree that little or no change has taken place in the relative distribution of sea and land since that epoch. All our main continents and islands not only existed then as they do now, but every year is adding to the amount of evidence which goes to show that so recent, geologically considered, is the glacial epoch that the very contour of the surface was pretty much the same then as it is at present."

We have not yet done, however. Dr. Croll does not argue that the warm equatorial current is diverted into the temperate and arctic seas by the direct action of the trade winds. He limits the direct action of those winds to first creating it and then pushing it bodily across the equator north or south, so as to confine it largely to one or other hemisphere, as the south-east or north-east trade winds prevail. It is quite clear, as he says, that the tendency of the trade winds is limited to impelling the intertropical waters along the line of the equator from east to west, and were those regions not occupied in some places by land, this equatorial current would flow directly round the globe. Its

Climate and Time, 9.

westward progress, however, is arrested by the two great continents, the old and the new.[3] It is the presence of these continents, and not the trade winds, which deflect the currents, and the latter move, after meeting with the barriers, along the lines of least resistance. The impetus, no doubt, which causes their movement is due to " the trades," but their deflection is due to the distribution of land and water.

As we shall show elsewhere, although there has been no change in this distribution since so-called glacial times which would shift the equator of heat to the south of the equator of the earth, there has been such a change (and a very slight one is needed) to affect the direction of some of the currents (after they have been deflected) by the land. What I wish to emphasize now is, that after the Gulf Stream has traversed the Gulf of Mexico and reached the Narrows of Florida, its course, instead of being from east to west, is from west to east, or rather from south-west to north-east; so is that of the main northern affluent of the equatorial current in the Pacific. That is to say, both of them reverse their direction at a certain part of their course and flow in a direction contrary to that of " the trades." Dr. Croll points out very clearly how this fact supports the theory of the wind-origin of ocean currents. He shows that in spite of the rotation of the earth, which would tend to give these currents a north-westerly direction, they flow eastward, because having reached the latitudes where the anti-trades blow from west to east, they are dragged along by them.[4] This is quite true, but it seems to me to be a very fatal argument for him to use. If the Gulf Stream, and the corresponding Pacific current, owe the larger part of their later motion, and their consequent spread into far northern latitudes, to the anti-trades, and if the present climate of the northern hemisphere depends upon the extent of this spread, as Dr. Croll allows, then it follows that more important factors than the trade winds, which give the equatorial current its western motion, are the anti-trades, which give the resulting currents their north-eastern motion. The amount of heat distributed in

[3] Climate and Time, 210.   [4] Id., 213.

high latitudes by these currents depends not on the strength of the trade winds but on that of the anti-trades. We can scarcely doubt that, as the blowing of these winds is very uniform, and is limited to an area where the parallels do not differ greatly in length, while their distribution is very regular, the force of the anti-trades is very nearly that of the trades.

From this it follows that when the north-east trades are strong the south-west anti-trades must be strong also. Dr. Croll's main position is that during the so-called glacial epoch the north-east trades were so strong as to force a larger quantity of water across the equatorial line into the southern hemisphere, but this would be at once compensated for by the fact that the stronger anti-trade winds in the northern hemisphere would give the current which they induced a greater impetus and a wider distribution than the same current when under the pressure of the trades. If the north-east trades were very powerful, so must the south-west anti-trades have been ; and if it were possible (as we have tried to argue it was not) for the former winds to push the equatorial current rather more to the *south* in the first part of its course, it must also have followed that the latter winds pushed them further *north* in the second part of its course, and the Gulf Stream must have been correspondingly faster and more widely diffused. In every way, in fact, in which we approach the problem from the meteorological side, we must conclude that Dr. Croll's theory fails to satisfy the drastic demands of the evidence.

Among the critics of Mr. Croll's theory was one who while accepting some of his premises deduced a different conclusion, namely, Mr. J. J. Murphy.[5] Mr. Murphy agrees with Croll that the glacial period occurred when the earth's eccentricity was at its maximum and the solstices fell when the earth was in perihelion and aphelion, and also that only one hemisphere at a time was glaciated; but he differs from him in urging that the glaciated hemisphere was that in which the winter occurred in aphelion and not in perihelion, or as he elsewhere puts it : " Mr. Croll thinks the hemisphere of *cold winter* will

[5] See Quart. Journ. Geol. Soc. xxv. p. 350.

be the glaciated one. I think, on the contrary, the hemisphere of cool summer will be the glaciated one," and he proceeds to argue that "it is a cool summer which most promotes glaciation, while a cold winter has usually no effect on it whatever." *Inter alia,* he says : "The dependence of the height of the snow-line on summer temperature and on the amount of snowfall, to the exclusion of winter temperature, may be best shown, perhaps, by two extreme cases. The mean temperature of the Altai mountains, according to Mr. Hopkins, is below freezing ; yet, in consequence of the comparatively warm summer, and the small snowfall, the height of the snow-line is about 6000 feet. On the Straits of Magellan, on the contrary, though the mean temperature is several degrees above freezing, the height of the snow-line is little more than half as much. It is well known that, other things being equal, the magnitude of glaciers depends on that of the snow fields in which they rise ; and as of course any depression of the snow-line will enlarge the snow fields, it follows that the lower the snow-line the further will the glaciers descend below it. As a decrease of about 3° F. is due to every 1000 feet of ascent in the hottest month, it follows that a fall of temperature to that extent in the hottest month would lower the snow-line by about 1000 feet ; and in many cases it is likely that the glaciers in such a case would descend 1000 feet further below the snow-line than at present, thus gaining a total increase of 2000 feet. This might not have much effect on the climate of Central Europe, but it would have a very great effect in those high latitudes where the glaciers would reach the sea and give origin to icebergs ; for we know that icebergs have great influence as transporters of cold. . . . The change in the eccentricity of the earth's orbit is in all probability amply sufficient to account for this or a much greater change in summer temperature.

"The sun's maximum distance occurs at present a little after the midsummer of the northern hemisphere. When it occurred at the same time of the year during the period of greatest eccentricity, the earth at our midsummer was receiving only nine-tenths of the quantity of heat which it now receives at that time of the year. I cannot calculate the effect on climate ; but it must have been very great, not only

directly by depressing the snow-line, but, as Forbes remarks, indirectly by chilling the air—and, I will add, by filling the North Sea with the icebergs, which must have broken off from the glaciers that filled the Norwegian fiords, as they do now from the glaciers of Greenland." Hence Murphy concludes that glaciation depended chiefly on a cold summer, but also, he says, on an increased snowfall, and this in the glaciated hemisphere he derives from the increased vapour created in the warmer hemisphere, which he concludes was condensed in the colder one, urging that at present the vapour raised in one hemisphere is to a large extent condensed in the other.

Mr. Croll, who is always at his best when criticizing the theories of other people, makes a very satisfactory reply to these arguments. He says : "I fear Mr. Murphy must be resting his theory on the mistaken idea that a summer in aphelion ought to melt less snow and ice than one in perihelion. The longer summer in aphelion, all things being equal, is colder than the shorter one in perihelion, but the quantity of heat received from the sun is the same in both cases. Consequently the quantity of snow and ice melted ought also to be the same ; for the amount melted is in proportion to the quantity of energy in the form of heat received. With us now, less snow and ice are melted in a cold summer than a warm one, but this is not a case in point, for during a cold summer we have less heat than during a warm summer, the length of both being the same. The coldness of the summers in this case is owing chiefly to a portion of the heat which we ought to receive from the sun being cut off by some obstructing cause.

"The reason we have so little snow, and consequently so little ice, in temperate regions, is not, as Mr. Murphy seems to suppose, that the heat of summer melts it all, but that there is so little to melt; and the reason why we have so little to melt is that, owing to the warmth of our winters, we have generally rain instead of snow. But if you increase the eccentricity very much, and place the winter in perihelion, we should probably have no snow whatever, and, as far as glaciation is concerned, it would then matter very little what sort of summer we had." [6]

[6] Climate and Time, 67.

We will now shortly turn to another modification of Dr. Croll's theory, which he has subjected to searching criticism, namely, that proposed by Mr. Alfred Wallace. Wallace agrees with Croll that a high state of eccentricity will produce on the hemisphere whose winter solstice is in aphelion a long and cold winter with a short and hot summer, and on the other hemisphere, whose winter solstice is at the same time in perihelion, a short and mild winter with a long and cool summer, and that precession will transfer these conditions from one hemisphere to the other. He also agrees that it is the physical forces set in motion by these causes, rather than the direct action of the causes themselves, which produced the glacial climate.

He differs from him, however, at a critical point. Croll argues that "the more severe the glacial condition of one hemisphere is, the warmer and more equable will necessarily be that of the other; for the very same combination of causes which would tend to cool the one hemisphere would necessarily tend to warm the other. The process to a large extent consists," he urges, " of a transference of heat from the one hemisphere to the other. Consequently the one hemisphere could not be heated without the other being cooled, nor the one cooled without the other being heated. The hotter the one the colder the other, and the colder the one the hotter the other."[7]   Elsewhere he follows this up, and says : " If the theory developed in the foregoing chapters in reference to the cause of secular changes of climate be correct, it follows that that long age known as the glacial epoch did not, as has hitherto been generally supposed, consist of one long unbroken period of cold and ice. Neither did it consist, as some have concluded, of two long periods of ice with an intervening mild period, but it must have consisted of a long succession of cold and warm periods ; the warm periods of the one hemisphere corresponding in time with the cold periods of the other, and *vice versa.*   It follows also from theory that as the cold periods became more and more severe, the warm intervening periods would become more and more warm and equable. As the ice began to accumulate during the cold periods in sub-

[7] Discussions on Climatology, 90, 91.

arctic and temperate regions, in places where it previously did
not exist, so in like manner during the corresponding warm
periods it would begin to disappear in arctic regions, where it
had held enduring sway throughout the now closing cycle.
As the cold periods in the southern hemisphere became more
and more severe, the ice would continue to advance north-
wards in the temperate regions ; but at that very same time
the intervening warm periods in the northern hemisphere
would become warmer and warmer and more equable, and the
ice of the arctic regions would continue to disappear further
and further to the north, till by the time that it had
reached a maximum during the cold antarctic periods, Green-
land and the arctic regions would during the warm inter-
vening periods be probably free of ice and enjoying a mild
and equable climate. The time when the ice had reached its
greatest extension on the one hemisphere would be the time
when it had disappeared from the other. " Here," says
Croll, " we have the grand crucial test of the truth of the
foregoing theory," that is to say, the theory that the glacial
epoch should have consisted of a succession of cold and warm
periods.[8]

With this view Wallace joins issue. He argues that the
effect of precession, as required by Croll, could only take place
when a country was *partially* snow-clad ; wherever a large
area became *wholly* buried in snow and ice, as was certainly
the case during the glacial epoch in Northern Europe, there
the glacial conditions would be continued, and perhaps even
intensified, when the earth approached nearest to the sun in
winter, instead of there being at the time, as Mr. Croll main-
tains, an almost perpetual spring.[9] Elsewhere he argues that
when the eccentricity and other conditions combine to pro-
duce a severe glacial epoch, precession will have very little,
if any, effect on the character of the climate as mild or
glacial. When the eccentricity becomes moderate and the
resulting climate less severe, then the changing phases of pre-
cession will bring about considerable alteration and even a
partial reversal of climate.[1] Again he says : " It follows
that towards the equatorial limits of a glaciated country

[8] Climate and Time, 236-237.
[9] Island Life, 503.　　　　[1] Id. 153.

alternations of climate may occur during a period of high eccentricity, while near the pole, where the whole country is completely ice-clad, no amelioration may take place. Exactly the same thing will occur inversely with mild arctic climates." [2]

The conclusion from this argument was of course diametrically opposite to that of Croll. " According to Wallace there could be no warm interglacial periods, either in temperate or polar regions, except during the commencement and towards the close of a glacial epoch." [3] " The occurrence of a mild interglacial period between two ice periods is directly in opposition to Mr. Wallace's view, that during a high state of eccentricity the ice would not disappear but be continued." [4]

Before turning to Croll's answer to Wallace's arguments, I would remark *in limine* that, if they are sound, I do not see how the glacial period, when once begun, was ever to stop. We have seen in the previous chapter that the direct effect of increased eccentricity upon climate is very small, and that such as it is, it is in the direction of increasing and not diminishing the amount of heat received from the sun. If, therefore, the snow and ice continue to accumulate in spite of hotter summers than there are in times of less eccentricity, I cannot understand why the glacial period should ever end at all. On the contrary, it would seem to follow necessarily that it must have gone on increasing in intensity, for the physical causes at work had, *ex hypothesi,* a cumulative force.

Turning now to Croll's answer.

Wallace argues thus : " Can we suppose that the mere change from the distant sun in winter and near sun in summer, to the reverse, could bring about any important alteration ?—*the physical and geographical causes of glaciation remaining unchanged. . . .* It seems to me quite certain that whenever *extreme* glaciation has been brought about by high eccentricity combined with favourable geographical and physical causes, then the ice sheet will *not* be removed during the alternate phases of precession, so long as these geographical and physical causes remain unaltered. It is true

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[2] Id. 154.　　　　[3] Discussions on Climatology, 91.　　　[4] Id. 135.

that the warm and cold oceanic currents, which are the most important agents in increasing or diminishing glaciation, depend for their strength and efficiency upon the comparative extents of the northern and southern ice sheets; but these ice sheets cannot, I believe, increase or diminish to any important extent unless some geographical or physical change first occurs."[5] Again he says: "What we have to consider is whether the alteration from a long cold winter and short hot summer to a short mild winter and long cool summer would greatly affect the amount of ice, *if the ocean currents remained the same.* The force of these currents is, it is true, by our hypothesis modified by the increase or diminution of the ice in the two hemispheres alternately, and they then react upon climate; but they cannot be thus changed till after the ice accumulation has been considerably affected by other causes."[6]

In regard to this argument Croll very justly says: "Here we have an impossible state of things assumed. It is assumed that it is possible for the winter solstice to pass from aphelion to perihelion and the *physical* causes to remain unchanged. It is assumed as possible that the astronomical conditions might be reversed without a reversal of the physical conditions."[7]

"When the winter solstice is in aphelion it sets in operation many physical causes, the tendency of which is to produce an accumulation of snow and ice; but when the solstice-point moves round to perihelion the tendency of these causes is reversed, and they then undo what they had previously done, and melt the snow and ice which they had just produced. If, as Wallace postulates, the physical causes remain unaltered in their mode of operation in spite of the astronomical causes having changed, there certainly will not be a change in the glacial condition, because the physical causes are much more powerful than the astronomical ones. The astronomical causes are perfectly unable, as we have seen, to produce a glacial state of things without the *aid* of the physical. How then could we expect that they could remove this glacial state, if the physical causes were actually

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[5] Id. 150.   [6] Id. 148.   [7] Id. 105.

working against them." [s] Croll goes on to urge that in thus basing his argument on a permanence of the physical causes concurrent with a change in the astronomical ones, Wallace is relying on a state of things which cannot possibly under the circumstances exist. "When the action of the causes is reversed, the effects will be reversed too, and as the physical conditions are dependent on the astronomical ones, it is clear that if the latter change the former must do so also."

This is not the only impossible condition relied upon by Wallace. In one of the quotations above referred to, he asks if the change from a long cold winter and short hot summer to a short mild winter and long cool summer would affect the amount of ice, *if the ocean currents remained the same*. But the ocean currents would not remain the same, argues Mr. Croll, and this he has little difficulty in proving, on the hypothesis that the premises upon which he and Mr. Wallace are agreed are true. He accepts Wallace's view, that ocean currents are mainly due to the difference in temperature of the polar and equatorial regions, combined with the peculiar form and position of the continents. The latter factor, he urges, has not altered, so far as we know, since glacial times. In regard to the former, Wallace argues that the only change that could produce the result would be such an increase of the antarctic ice as would increase the northern flowing currents sufficiently to melt the arctic ice, and this he considers impossible without some geographical change. To this Croll replies that here is a double mistake, and that we can readily understand how all the antarctic ice might disappear under the influence which would be brought to bear upon it were the eccentricity high and the southern winter solstice in perihelion. The warm and equable conditions of climate which would then prevail, and the enormous quantity of intertropical water carried into the Southern ocean, would soon produce a melting of the ice. Layer after layer would disappear, and as soon as the weight of the sheet became less than that of the water which it had displaced, the sheet would float. After this it would no doubt shortly break up and become dispersed. [9]

[s] Id. 105, 106.   [9] Id. 111–112.

After the previous paragraphs were written, there appeared Sir R. Ball's work on "The Cause of a Glacial Age," in which he has revived views which I thought had been finally laid to rest by the arguments of Croll and others. Dr. Ball professes to base his conclusions on the fact that the proportions of heat received in each hemisphere during summer and winter are as 63 to 37. With this I have no quarrel whatever. I cannot find in his book, however, the link connecting this postulate with the conclusion, for he goes on to admit that the proportions named are constant, whatever change there may be in the eccentricity of the earth's orbit, etc. Being constant, it seems to me that he adds no element of any kind to those present to Croll, Meech and others; and I cannot see how a constant factor can develop a variable tendency. The efficient variable factor, from which in fact Dr. Ball argues, is really the same as that which was relied upon by previous writers, namely, that (to use his own words) *the irregularity* in the supply of heat to the earth is capable of producing great climatic changes, and he calculates accordingly the daily aggregate of heat received in summer and winter respectively during times of great and of small eccentricity. This involves a patent fallacy.

As we have seen, the creation of a glacier, or of a glacial period, is really a process of distillation on a great scale, in which a sufficient supply of vapour in the air is quite as important a factor as a sufficiently powerful condenser to convert it into snow and ice, and the processes of making the vapour and of condensing it must be going on at the same time in different areas. We cannot condense the vapour made in summer by the winter's cold, because we cannot store up that vapour. Vapour is lighter than air, and air charged with vapour inevitably and immediately rises until it reaches a higher altitude, and there it finds greater cold, and inevitably discharges its moisture, generally in the shape of rain, and this is the cause of the great rainfall in the tropics.

In the case supposed, therefore, a hotter if shorter summer would no doubt evaporate more water per day, but vapour thus made would descend in the shape of rain long before the winter came on; and when it came, although there would un-

doubtedly be greater cold to condense what vapour there was, the amount of vapour left in the air would be very slightly, if any, more with a great eccentricity than with a small one.

Not only so, but inasmuch as all parts of each hemisphere would share in the changes of temperature simultaneously, it follows that while the equator was made hotter in summer, so would the polar area, and if this change tended to increase evaporation at one end of the machine, it would also impair the efficiency of the condenser at the other by making it warmer. On the other hand, while the condenser was made more efficient in winter by being made colder, the increment of cold would extend also to those regions where the vapour was being made, and thus diminish the evaporation; so that there would be a compensation all round, and, so far as we can judge, a variation in the eccentricity of the earth's orbit would have little or no appreciable effect on the snowfall.

But granting that it had, this would not tend to create a glacial climate unless some portion of this snow accumulated. Dr. Ball admits frankly that " the formation of an ice-sheet is an operation of such magnitude that it is wholly impossible for it to arise as the product of a single season. We cannot believe," he continues, " that the ice-sheet is manufactured afresh each winter, and dissolved away during the ensuing summer; it must slowly grow from year to year, perhaps we might say from century to century, the net annual increase being determined by the excess of ice produced during the winter over the ice melted during the ensuing summer." [1] This is very true and well put. He then goes on to state the difficulty to be met. " If a certain daily average of heat, given uniformly, be quite incompatible with the accumulation of ice, we have to explain how the ice-sheet does wax great when the same annual total of heat is given, but in such a way that each day in winter receives much less than its average, and each day in summer much more than its average." [2]

I do not think this is quite ingenuous. We have no glacial climate at this moment, and yet we have precisely the same disparity in the amount of heat between summer and winter

Op. cit. 110.   [2] Id.

that we should have if the eccentricity of the orbit were at
its limit. We have no uniformity of distribution now, and
what really has to be explained is, how an addition to the
number of days over which the winter's heat is distributed
from an excess of 7 to one of 33 (this being the extreme of
variability possible, and only possible after portentous ages),
would create a glacial period. Now under the conditions
postulated, I know of no reason whatever why the longer
winter's snow (even though its fall was greater than we have
shown it to be probable), should resist the more concentrated
and fiercer heat of the corresponding summer. It seems to
me inevitable that it would not ; and anyone who has read
what happens on the Siberian tundras, in the neigbourhood
of Yakutsk, where the coldest winters in the world are found,
when in a very short time the fierce sun of the short summer
clears away every bit of snow and allows the ground to be
carpeted with flowers, will have a very good object-lesson in
view of what we are arguing about ; for it is not suggested
that even with the greatest possible eccentricity there would
be anything like the sharp and extreme contrast between
winter and summer which occurs at Yakutsk, and one or two
other exceptional places.

Sir R. Ball nowhere, that I can find, meets this issue except
by an illustration which is surely meant as a joke only.
" Suppose," he says, " a horse has a daily allowance of a stone
of oats, which is sufficient to keep him in good health ; the
condition of that horse would be surely very seriously affected
if he received a stone and a half for the first six months, and
but half a stone per day for the remainder of the year ; yet
in this case, as in the former one, his daily average of a stone
per diem has not been interfered with." That a horse which
is overfed for six months and underfed for another six will
probably be ill, is true enough. That this proves that a hot
furnace will not in a few hours melt the ice accumulated by
a frost of several days, is a specimen of transcendental logic.

I can see very little in what follows, namely, that inas-
much as the earth cannot store up its summer heat to carry
it over to the next winter, therefore this heat cannot be utilized
to equalize climates. Empirical tests show that it does store it
up most effectually by converting snow and ice into water,

and by melting at a tremendous pace the snowy mantle of the polar islands, of the barren grounds, and the Siberian tundras, until they are bared of snow, not in the course of a summer, but of a week or two of fierce continuous sunshine ; and I can see no warrant whatever for the dictum that " the sunbeams, in the brief and fiercely hot summer of the glacial period, failed to melt as much ice as had been accumulated during the preceding winter, and that it was thus the ice-sheet was permitted to grow from year to year until the stringency of the astronomical conditions became relaxed." [3]

I am bound to say, therefore, that I can trace in Sir R. Ball's reasoning no assistance whatever in finding an astronomical explanation for an ice age.

This completes our survey of the meteorological theories which have been forthcoming to explain the glacial period. I know of no others. They have been elaborated by some of the acutest and keenest-witted of critics, and so far as we can see they utterly fail to meet the facts and the requirements of the case. They are in effect as fragile as the astronomical theories, and cannot be made a basis for supporting the gigantic conclusion of a glacial period. The appeal of the geologist to the astronomer and the meteorologist must therefore be held to have gone by default, and we are remitted back to the position that—if there was a glacial epoch, the explanation of it, as C. Martins says in the sentence I have quoted at the head of this chapter, must be the work of future generations.

[3] Id. 111.

CHAPTER XI.

THE GEOLOGICAL WITNESS AGAINST RECURRENT ICE AGES AND

WARM INTERGLACIAL PERIODS.

" Les observations se multiplient, les faits se compliquent, et suivant
une pensée de Montaigne, on pourrait dire que plus on découvre et plus
on doute."—LE HON, " L'Homme Fossile," 351.

The general evidence of palæontology and palæophytology as to
former glacial epochs decisively against the notion—The inor-
ganic evidence at the most points to the former existence of local
glaciers—Cambrian and Silurian beds : Wright, Murchison,
Fielden—Devonian beds : Lyell, J. Geikie—Carboniferous strata :
Fielden, Stirrup, A. Geikie, Prestwich, Ball, Lyell, Wright,
W. T. Blandford, Searles Wood, jun.—Permian beds : Lyell—
The Secondary Rocks ; general testimony of the biological remains :
Lyell and Wallace—Triassic beds : Pengelly, Von Cotta—Jurassic
beds : Judd, Wright—Chalk : Lyell, Gunn—Evidence of the Tertiary
beds : Wallace—Imperfection of the geological record : Wallace and
Wright—Eocene and Miocene beds : Searles Wood, Judd, Lyell,
Nordenskiold, Wallace, Dawkins—The beds of the Arctic regions
especially conclusive : Nordenskiold, Wallace—General conclusion :
Lyell and Le Conte—Interglacial beds : Croll's demands ; general
arguments against—The Swiss evidence : Penck, Heer, Lyell,
C. Gras, Mortillet, Escher Von der Linth, Favre—Italian beds :
Gastaldi—North German beds: Credner, etc., Neumayr—Unanimity
of French geologists on the subject—England, Lamplugh, H. Wood-
ward, C. Reid, and J. M. Reade—Scotland : C. Reid, Somervell—
American evidence : Gilbert, Chamberlin, Wright, Whittlesea—
General conclusion.

THE failure of the astronomer and the meteorologist to
justify the tremendous appeals of the recent school of ultra-
glacialists naturally invites a closer criticism of the geological
data upon which its claims are based. Do the facts of
geology therefore demand that we should invoke periodical
changes of climate in past times ? Do they give such a support
to the theories of Agassiz, Croll and Geikie that we are
bound, notwithstanding the verdict of astronomy and meteoro-
logy, to support them ? I believe they do nothing of the
kind, and that no hypotheses that have fascinated so many
scientific men, and pervaded so many scientific discussions,
have ever been based upon more fragile evidence.

The best barometer we can use to test the character of a climate is the fauna and flora which lived while it prevailed. This is not only the best, but is virtually the only barometer available when we inquire into the climate of past geological ages. Other evidence is always sophisticated by the fact that we may be attributing to climate what is due to other causes; boulders can be rolled by the sea as well as by sub-glacial streams, and conglomerates can be formed by other agencies than ice. But the biological evidence is unmistakeable; cold blooded reptiles cannot live in icy water; semi-tropical plants, or plants whose habitat is in the temperate zone, cannot ripen their seeds and sow themselves under arctic conditions. When, therefore, it is argued that the so-called glacial period was a recurrent phenomenon, we ought to find unmistakeable evidences of it in the palæontological record, and if we fail to find it, it seems to me, we have destroyed the very basis of the whole theory. As a matter of fact such evidence is virtually wanting. We may examine the whole series of geological horizons, from the earliest palæozoic beds down to the so-called glacial beds, and find, so far as I know, no adequate evidence of discontinuous and alternating climates, no evidence whatever of the existence of periods of intense cold intervening between warm periods, but just the contrary. Not only so, but we shall find that the differentiation of the earth's climate into tropical and arctic zones is comparatively modern, and that in past ages not only were the climates more uniform, but more uniformly distributed over the whole world. These are most stubborn and desperately difficult facts to get over. How are they got over ? By an appeal to that very convenient scapegoat, the imperfection of the geological record. This seems to me in this particular instance to be a more than usually audacious recourse to human credulity. Why the products of warm climates should be preserved and those of cold ones be destroyed, I know not. Surely the arctic shells that occur in many places in glacial beds, and the bones of reindeer and musksheep which have been found in many localities at the same horizon, have not had a greater difficulty in being preserved than such fragile débris as we find in so many Tertiary strata. I cannot understand how it can be seriously argued that the great

denuding agencies, to which the imperfection of the geological record is attributed, should have been able to discriminate between arctic shells and semi-tropical ones, and should have preserved for us the bones of animals which loved warm surroundings while they destroyed those which loved cold ones, and this not merely in temperate latitudes, but in the arctic zones themselves. Assuredly the appeal to the imperfection of the geological record in such a case is the last straw which the drowning theory clutches at. I have said that I look upon the palæontological evidence as absolutely conclusive on the question. I do not wish, however, to evade evidence of another kind.

This evidence consists almost entirely in the existence at certain horizons of conglomerates or detached boulders of rocks foreign to the strata in which they occur, and which, it has been argued, can only have been transported by ice. This argument is to me most fallacious. What water can do in transporting enormous blocks to great distances may be seen in the accounts Abich has given of the floods in the Caucasus, and others have given of floods in the Himalayas, etc., to which I shall refer in a later chapter. So that the mere presence of large boulders seems to be not only inconclusive but misleading. To make sure that the agent was ice, we ought to have the proper marks of ice action, striated and polished, moutonnée surfaces, boulders bearing not merely a round and water-worn contour, but flat sides and with scratches on them. These special marks of ice are very rare and often quite wanting. Again, if we are to postulate an ice age, we must not be content to find a few sporadic water-worn stones here and there, but widespread and unmistakeable traces, ranging over many latitudes.

Let us remember what kind of evidence we ought to have if we are to believe in a recurrence of such conditions as are evidenced by the drift phenomena. These phenomena need no microscope to discover them. They are among the most stupendous and the most cosmopolitan monuments furnished by geology. Within the geographical limits of the drift phenomena we can hardly examine a rood of ground without finding traces of them, either in the striated boulders, scratched rocks, clays, muds, or in the animal remains. They are

present everywhere, and most of them are among the most indestructible of witnesses. Where can we find at any earlier geological horizon facts to parallel these? The earth has been diligently searched for them, but with the exception of a few uncertain and sporadic and dubious cases of the occurrence of some boulders in old conglomerates, which are compatible with other explanations, where are we to look for evidence? What a strange thing is this! If the so-called glacial age has been a recurring one in all geological time, assuredly we ought to find at every horizon not merely evidence, but unmistakeable proof. We have not to do with fragile materials which could be destroyed by denudation, but with tough boulders, with rounded land-surfaces on crystalline rocks, etc., etc. Nowhere, except in the very local instances to be presently referred to, is this forthcoming, but the contrary. The evidence of the animal and vegetable débris again, of which we have so much from every horizon, is perfectly consistent with that of the lithology, and consistent in protesting against a theory which makes so many demands on our credulity.

It is true, as we have seen, that traces on a larger scale have been found in the Permian beds of Europe and the Carboniferous ones of India, the Cape, and Australia, which have led a good many geologists to argue ice action on a considerable scale; but ice action does not mean the existence of a glacial period. Glaciers probably always existed as they exist now where the land was sufficiently high, and we ought to find traces of them in the geological record. What is wanted, however, is not testimony to sporadic glaciers or local ice action, but to widespread glacial phenomena such as would witness an ice age.

Of this I can find no evidence whatever.

Let us now turn to the facts and see how they support the conclusions here enunciated.

In regard to some very old conglomerates reposing unconformably on Laurentian rocks in Tennessee and North Carolina, which, although no scratched boulders have been found in them, Professor Shaler explains as the result of ice transport, Professor Wright says, " Along the whole Appalachian border there were formerly Archæan highlands of indefinite

height, of which the stumps alone now remain in the present hills and mountains. . . . For all we know, the material spread over this area of rudimentary rocks was all within reach of rivers coming down from Archæan heights, and so there is no necessity for supposing extensive glacial transportation from more northern water-sheds, such as we are compelled to suppose in the glacial age of recent date. The same remark applies to all the evidence concerning a succession of glacial periods. In all cases they are of such limited character that local glaciers coming down from isolated mountain masses, such as now come down from the mountains of Alaska, Patagonia, and at no very distant date from those of New Zealand, are sufficient to account for the facts." [1]

In regard to the older palæozoic beds, the evidence of ice action which has been quoted is altogether very obscure and very slight, and Mr. James Geikie admits that we look in vain for Cambro-Silurian *roches moutonnées* and boulder clay or moraines ; and he argues that it is not reasonable to expect such evidences, since it would appear that for the most part the earth was submerged at this period, and there was very little subaerial land. Granting this, what should we expect to find ? Why assuredly some very marked evidence among the super-abundant submarine remains to attest oscillations of tem-perature, such as we can find in the Caithness clays compared with the modern beaches. Instead of this we find a very uniform fauna spread over the whole earth, pointing to very uniform conditions.

Murchison says, " A glance at the tables of ' The Thesaurus Siluricus ' will show the groups of fossils which are common to the most distant centres, and we thus ascertain that some of the *species* even, are common to Britain, Scandinavia, Bohemia, America and the Antipodes in Australia. These data prove that the Silurian system of life had an almost universal spread."

If we ignore mere species and look at the larger groups, the fact is still more striking. To again quote Murchison : " Many Trilobites, Cephalopods, Brachiopods, Cyclideans and Corals of analogous forms were spread out over areas at enormous distances in the earlier primæval times, proving that oceans

[1] The Ice Age in America, 438.

containing similar groups extended from China and the Himalayan Mountains over Siberia and Russia to Western Europe, and it is enough for me to feel assured that the various associates of the Calymene Blumenbachii, or any other well-known Trilobite, must have required just the same temperature and surrounding media in whatever part of the world they lived." [2]

This is assuredly true; but these conditions are exactly the opposite of those which Dr. Croll and his followers demand, namely, intense cold in one hemisphere and mild, genial temperature in the other. Not only do we find this uniformity, but we find also that the general temperature of the ocean, even in very high latitudes, was high, more like that of the tropics than of the present Arctic Seas. On this subject I should like to quote some remarks of Colonel Fielden, who, after referring to the fact that Silurian rocks extend over the Parry Archipelago, North Greenland, and Grinnell Land, Spitzbergen, and Nova Zembla, continues : " Mr. Etheridge [3] thus refers to the conditions of that Palæozoic sea, as exemplified by its fossil Sclerodermic Corals. These undoubted reef-forming corals of the Silurian epoch were just as much inhabitants of warm water in northern latitudes at that period as are the Sclerodermata of to-day in the Indo-Pacific and Atlantic Oceans; and as we know of no compound coral that will exist at a lower temperature than 68° F., and as the surface water under the equator in the Pacific has a temperature of 85° F., and in the Atlantic 83° F., it seems clear that the range from 68° F. to 85° F. is best adapted to and not too high for the growth of the reef-making species. We may fairly assume that the temperature of the Polar waters during Palæozoic times was as high as that of the Indo-Pacific and Atlantic now, where coral reefs abound. We are not justified in supposing that the laws regulating oceanic life were very different then from those now existing in the same groups under the equator or between the tropics. These corals were forms of life which must have been tropical in habits and requirement."

As a good proof of the special pleading adopted by

[2] Siluria, 502.   [3] Quart. Journ. Geol. Soc. 1878, 578.

the advocates of extreme glacialism, I may quote the
following sentence from the "Great Ice Age." Speaking
of the boulders in the Lower Silurian of the North of
Scotland, cited as evidence of a Silurian ice age, Mr. Geikie
says: "No ice markings, however, have been observed
upon any of the stones, nor when we reflect upon the vast
age of this deposit, could these be expected to have re-
sisted the long-continued action of percolating water." [4]   That
is to say, these boulders which are cited to prove an ice age
bear no trace of ice action, and, *mirabile dictu*, we are told it
is unreasonable to expect any trace, since it would long ago
have been worn away by percolating water.   What a curious
form of proof this is !   But apart from this we nowhere
find that percolating water destroys or blunts the sharp clear
marks of ice which occur on all recently exposed surfaces in
glaciated districts.   Why should it have cleaned off such marks
altogether from the old surface during the long time since
the Silurian days when it has not worn away the sharp-edged
trilobites' armour, or the sculpturings on the brachiopods, etc.

This being a description of the condition of things in the
arctic regions in Silurian times, and, so far as we can judge
from the palæontology, the constant and persistent state of
things, how are we to postulate the intervention of glacial
periods, which, if Dr. Croll's theory be true, must have been
much more intense than now, since there was so little land to
interfere with the drifting of the warm ocean currents from
one hemisphere to the other ?   The conclusion seems inevit-
able.   How then are we to reconcile the existence of these
wide-spread tropical corals with the contemporaneous existence
of a palæocrystic sea, and how explain the complete absence of
arctic forms among the remains ?

Let us now turn to the Devonian strata.   Here again we
find the same evidence of uniform and high temperature.   As
Sir Charles Lyell points out, the Ganoid fishes, which charac-
terize this horizon, have their analogues in the African
Polypterus, while the vast Crustaceans are most like the
forms found in the Japanese seas.   The molluscs, corals,
and plants are very like those of the Carboniferous beds,
and point to a similar warm climate.   These beds full of

[4] Op. cit. 568.

rich life have been found as far north as 49° in Canada. Here then we have the same palæontological difficulty, while the evidence on the other side is very unsatisfactory. I will give a sample from each of the two typical localities where traces of a Devonian glacial age are supposed to have been found. In the district of Kirkby Lonsdale, "Professor Ramsey," says Lyell, "has himself adverted to the fact, that the conglomerate just alluded to has been subject to violent movements in different directions, and to great pressure after it was buried under thousands of feet of Carboniferous strata. In consequence of these movements some markings have been produced within the body of the rock itself, one pebble having occasionally been squeezed and forced against another so as to indent it. Many of the pebbles also, and stones two feet and more in diameter, have acquired that polish which is called slickenside; and the same may be seen in many parts of the marly matrix, and even in the layers of carbonate of lime which have here and there been deposited in the interstices between separate stones. Scarcely in any district of England has there been a greater succession of rents and faults, and it is not a little difficult to decide in many cases to what kind of mechanical action many of the effects alluded to may have been due."[5] Assuredly, with such contingencies, the discovery of a few boulders with scratches in their beds, form a fantastic postulate to hang a glacial period upon. If we turn to Scotland, Mr. James Geikie has very frankly pointed out the difficulty. "The Devonian breccias in the Lammermuirs have," he says, "been clearly derived from the Silurian beds of the intervening high grounds, which are not more than eight or nine miles off, and which are only 1700 feet high. Under present geographical conditions, it is doubtful whether perennial snow and glaciers of any size at all could exist in the region of the Lammermuirs at a less altitude than 7000 feet or more. But between the breccias of Haddingtonshire and the equivalent deposits in Berwickshire there is no space for any intermediate range of mountains of circumdenudation of such a height, and under the extremely uniform conditions of Palæozoic times the snow-line could

[5] Lyell's Principles, 230-231.

not possibly have been attained even at that elevation. When the Devonian coral reefs, described by Dupont, were growing in the sea that overflowed Western Europe, to what height must the southern uplands of Scotland have been elevated in order to reach the snow-line ? We may make what allowance we choose for the denudation, which the Silurian rocks of the Lammermuirs must have experienced since the deposition of the old Red Sandstone ; but it is simply a physical impossibility that mountains of circumdenudation of the desiderated height could ever have existed in the Lammermuir region, at the time the coarse breccias were being accumulated."[6] Mr. Geikie then goes on to invoke a lowering of temperature, due to increased eccentricity, as if this lowering of temperature to the extent required would not have been immediately evident in the fauna and flora.

Again in regard to the boulders quoted by Mr. Etheridge as proof of ice action in Devonian times, in Australia, we are told that " grooved and scratched stones have not been observed, but from similar deposits on the Lerderderg river, Victoria, Mr. Daintree obtained a few grooved stones."[7] If the boulders are simply rounded stones without scratches, they clearly have nothing to do with ice action.

If we turn to the Carboniferous period we fare no better. This period, marked by the profusion of tree ferns, points to a damp, warm, and equable climate, such as we now find in the islands of the tropics. This is also evidenced by the abundance of gigantic Lycopods (Lepidodendra), and Cryptogams (Sigillariæ). The same conclusion may be derived from the reptiles of the coal, which are confined to two orders, Ganocephalæ and Labyrinthodontia, apparently amphibians allied to the Salamander. Their modern representatives are most numerously represented in the Southern United States, and in Mexico. The prevalence of cephalopods, encrinites, and stony corals whose modern relatives are also found to flourish in the lower latitudes, points the same lesson.

They were not thus limited in Carboniferous times.

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[6] J. Geikie, The Evolution of Climate, 15, 16.
[7] Report Geol. of the district of Ballarat, Victoria, 1866, p. 11.

Captain Fielden refers to the great development of carboniferous limestone, with its abundant fossil remains, which he found in the most northern part of Grinnell Land, between 82° 43′ and 82° 50′, and consequently the nearest known fossiliferous rocks to the North Pole. He tells us the strike of this limestone formation of Fielden Peninsula and Cape Joseph Henry is towards Franz Joseph Land and Spitzbergen, from which latter country a rich fossil fauna of the same age is known, and therefore there can be no question that carboniferous rocks must likewise underlie the Polar Sea.[8]

Coal strata with similar fossils have occurred all round the Polar basin. They have been found on Melville Island in lat. 75° N., at the mouth of the Lena, and on Bear Island, in 74° 36′ N. They occur in America from Alabama, in lat. 30° to the arctic regions, and in Europe from Central Spain, in lat. 38°, to Scotland, in lat. 56°, and may be said therefore to have occupied a continuous cap round the North Pole.

While the remains in the coal beds point *uniformly* to warm conditions, there is no biological evidence whatever pointing to a cold period.

In regard to the notion of Mr. Croll, that the coal flora is the growth of an interglacial period, Mr. Stirrup urges that the character of the vegetation during the whole of the long period embraced by the coal measures, which is identical in character, and indicates no change of climate during the millenniums of millenniums represented by the thousands of feet of thickness of the coal measures, precludes this view. As to the boulders found in the coal beds Professor Dawkins says : " It seems to me that the ice theory is a theory which would hardly account for the problem of the existence of these stones in the carboniferous strata. For us to imagine that there were fleets of icebergs careering over this country during the time when those wonderful carboniferous forests were flourishing, seems to me altogether a mistake, and quite impossible. The vegetation is that of a warm climate. We also have other evidence, besides the trees, of the same fact, namely, the enormous spiders, like the tree

[8] President's Address, Trans. Norwich Nat. Hist. Soc. iv. 150-151.

spiders of Brazil, and also scorpions.[9] It seems to me that these carboniferous boulders have no signs about them of having been shaped or transported by ice." Though they differ in form and size, they are smoothed, often polished, with the corners rounded off by abrasion ; their forms are various—roughly quadrangular, irregularly ovoid or elliptical, occasionally globular—and all have been evidently water-worn. Their surface does not exhibit lines and scratches, such as those seen on boulders from the glacial drift. "Irregular furrows," says Mr. Stirrup, "and markings, it is true, may be detected on some, but I take them to be attributable more to the structure of the rock, or in some cases probably to pressure, rather than markings due to ice; a thin film of coal or slate often adheres to them, and has the appearance of the polished surface known as slickensides, which had probably been produced by the great pressure to which they have been subjected."[1] In appearance, shape, and other characters, therefore, these carboniferous boulders have nothing in common with the flat-sided, striated boulders of the till.

Their mode of occurrence is equally against this origin. They are found singly or very sporadically, generally imbedded in the coal itself, which could not have grown in water frequented by ice. Weiss, who has written a memoir on the stones of this age from the Continent, attributes their portage to floating trees, and he adds that at that time there were certainly trees big enough to carry loads of several hundredweights without sinking, and he says they may either have got entangled in their roots or fallen down on rafts of drift wood, of which a large part of the coal measures seem to consist.

In the first volume of the " Geological Magazine," pp. 25, 26, etc., is a description of an angular piece of greenstone weighing six or eight pounds, placed vertically in a bed of fire clay in the coal measures at Burnt Island. It rested on the coal seam. The layers of fire clay for a couple of inches from the bottom were bent sharply down, and the part of the coal seam above it was compressed. The layers above the stone showed no trace of disturbance. . . . The block, says

[9] Trans. Manch. Geol. Soc. xix. 422, 423.
[1] Stirrup, On Foreign Boulders in Coal Seams, 3, 4.

Sir A. Geikie, must have come into its present position after the deposition of that portion of the fire clay it squeezes down, and before the accumulation of the part which is heaped up over it, *i.e.*, when the bed of fire clay was half formed. Sir A. Geikie considers that neither water nor ice carried it to its present site, but that it was in fact a volcanic bomb ejected from a neighbouring crater and dropped into its present place, there being other traces of volcanic action in the neighbourhood.

Prestwich argues that the boulders found in the French carboniferous beds may have been washed down at that period by the torrents from the adjacent granite mountains.[2]

Professor Ball, in regard to the angular fragments found in the carboniferous limestone of Dublin, says : "I believe all who carefully examine the specimens will agree that they exhibit none of the indications of the existence of glacial conditions such as we might reasonably expect to find. The fragments, so far as I know, are invariably angular, and show no marks of having been acted upon by ice, or even of having been rolled by water ; they look as if they had been freshly broken or torn off the rocks from which they were derived. Although some of them rest upon the earthy parting in the limestone, their environment is strictly calcareous, consisting largely of encrinite stems, and there is an absence of the argillaceous silt so often conspicuously present. I am therefore unwilling to accept any iceberg or ice-raft theory of transport." He goes on to suggest their transport by sea-weeds. In the discussion Mr. de Rance said angular fragments of limestone were found attached to the roots of *Laminaria* at the Orme's Head in North Wales, while De E. Percival Wright referred to Anson's mention of the occurrence of sea-weeds out at sea with stones attached to them.[3] Elsewhere Mr. Ball says of these same boulders: "The specimens exhibit none of the indications of glacial conditions, whether we regard the character of the boulders or the nature of the rocks in which they are imbedded."[4] In regard to the boulders in the Potsdam rocks, Lyell says : "They may have

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[2] Quart. Journ. Geol. Soc., August, 1887.
[3] Q.J.G.S. xliv. 373-4.
[4] Geol. Mag. decade III., v. 232.

been derived originally from the destruction of rocks in the immediate neighbourhood, as they consist of the same gneiss as that on which they rest, and therefore cannot be said, like certain modern erratics in Canada, to point to glacial action or to imply that large blocks were transported by icebergs at a very remote period." [5]

Turning to the carboniferous conglomerates of the Appalachian range, which Professor Shaler attributes to ice action, Dr. Wright says : " For the most part, the pebbles of this conglomerate consist of quartz or quartzite, well rounded, and seldom of larger size than can readily be transported by water. . . . Altogether, it seems more likely that we have in these conglomerates underlying the Appalachian coal fields of America, the wash brought down by large rivers heading in the mountain plateau from the north and west. . . . That floating ice may have played some part in the streams coming down from the mountain heights is not improbable ; but it is doubtful whether the facts warrant us in inferring anything more." [6]

Speaking of the Talchir boulders in India, Dr. W. T. Blandford says that their rounded condition seems quite opposed to the idea of transport by true glaciers. " *Indeed,*" he adds, " *we have here no evidence whatever of the action of glaciers, the deposit differing greatly from the glacial drift and Permian breccias of England.*" [7] In this view Mr. Bauerman also concurs.[8]

Arguing on more general grounds in regard to the coal measures, Mr. Searles Wood, Junior, says that if Croll's view be right, we ought to find, not merely conglomerates and other beds at distant localities occasionally presenting features supposed by some to indicate ice action, but the very coal measures themselves, which separate the successive coal seams in this and other countries, and which are those referred to intervals of glaciation and submergence, invariably abounding with evidences of ice action. We have not, however, heard of this being the case. Mr. Croll, anticipating this objection, endeavours to explain it, on the ground that the carboniferous

Lyell, Travels in America, ii. 125-6.
[6] The Ice Age in America, 437.
[7] Records Geol. Survey of India, i. 49, 50.
[8] Q.J.G.S. xxxi. 542.

land was too low and too flat to allow glaciers to form; and that for the same reason there would be no rocky cliffs to supply boulders for coast ice to transport and distribute. Considering, however, that the Silurian system had been upheaved and penetrated by eruptive rock before the Carboniferous period, and that yet older rock masses than the Silurian had been also for ages in existence, and these at no great distance from the coal-fields of Britain, such an explanation demands greater concessions than most geologists will be willing to grant.[9]

We will now pass on to the Permian beds. In regard to their fossils, Lyell says : " Amongst the Permian shells we find the genera Nautilus and Orthoceras, and these are sometimes accompanied by large reptiles of a family called Thecodonts, which combine in their structure many characters of the living crocodiles and lacertians. They are most nearly allied to the Varanian Monitors, which now inhabit tropical countries. The fossil plants of the Permian formation are very like those of the carboniferous beds, and indicate the prevalence of a warm and moist climate throughout a great part of the northern hemisphere." [1] The Permian beds have been traced as far north as Petschoraland in Russia, between lat. 65° and 70° N.[2] The climate thus defined in the latitudes here described is assuredly inconsistent with there having been contemporaneously a glacial epoch going on. The fact that large angular boulders have been found in Permian beds in several places makes the case for the existence of ice action in Permian times stronger than that of any other period, and yet the evidence is not very satisfactory.

In the Permian at Ifton, in Shropshire, there occur seams of coal. From the identity of the age of these coal seams with the Permian conglomerate of Alberbury, only fifteen miles off, Mr. Davis, F.G.S., questioned the probability of that conglomerate being evidence of glacial age, since two climates so dissimilar as a glacial one. and that in which a carboniferous flora flourished could not exist together within so limited an area.[3]

Sir Charles Lyell says, " I have visited a great many of the

[9] Geol. Mag. 1876, 397.
[2] Id.
[1] Principles, i. 222.
[3] Geol. Mag. 1873, 46.

localities where these Permian breccias occur. . . . I scarcely ever found a large fragment *in situ,* and never one, large or small, with a polished and striated surface. . . . a proof that they are by no means common." [4] Lyell nevertheless adheres to Ramsay's view, except that he quotes the stones as evidence of glaciers having existed not far off, and not as evidence of an ice age. In regard to the existence of glaciers in Permian times, the fact is not only possible, but if there was a good deal of high land at the time, it is probable that they would exist as they exist now. To their existence I raise no objection at all. What I demur to is that anything as yet discovered in Permian breccias justifies the postulate of *an ice age.*

Speaking of the Palæozoic beds generally, Murchison says: "Admitting for a moment the applicability of this theory of Mr. Croll, would not, I ask, the intervention of ice action in warm, humid and equable periods have left some trace in the natural history products of such colder fits? Ought we not to meet with some animals and plants indicative of such cold climates? But as no signs of this sort have been detected in Palæozoic rocks, I cannot admit that a few striated stones or erratic blocks, detected at wide intervals only, in some old conglomerates, can in fairness be adduced as indications of such grand and general changes of climate in those olden days, when the seas of our planet and its lands contained, as far as observation goes, no living thing which does not bespeak a moderate and moist if not a warm climate." [5] Von Cotta has made a similar protest.

Let us now turn to the Secondary rocks. The evidence of these is equally forcible, and on the palæontological side it is assuredly overwhelming. Lyell says: "We find a very general agreement among zoologists and botanists, as to the warmth of European latitudes in the Oolitic and Triassic eras. The vegetation of these periods consists chiefly of cycads, conifers, and ferns. Professor Heer remarks that the tree which is most common in the upper trias in Switzerland, has a near affinity to a living African species of Zamia, and M. Adolphe Brongniart had long before expressed his opinion that the plants of the Secondary period favour the

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[4] Principles, i. 223.   [5] Siberia, 548.

hypothesis of a climate like that of the West Indies." The same author goes on to urge that the best proof of this warm climate is the great abundance of reptiles, which cannot exist under very cold conditions, and are remarkably absent or become quite insignificant in temperate regions. "There is no area on the globe at present," says the same writer, "between the parallels of 40° and 60°, where a climate exists like that which we may have supposed to have prevailed when the triassic and oolitic rocks were formed ; and he argues that the nearest conditions are to be found in the Galapagos Islands, which are placed under the equator, and where great tortoises, lizards and snakes abound. The rich marine fauna of the St. Cassian beds in the Austrian Alps, shows by the large size of its ammonites, orthocerata, and other shells, that in the east of Europe the seas enjoyed a warm climate at the same time that in the west the triassic reptiles were swarming on the land, and in the rivers and estuaries."

The wide-spread distribution of these beds and their contents is another remarkable feature. The same genera and even species of ammonites and other shells proper to the oolitic strata of Europe, occur also in beds of the same age in Sinde and Cutch in lat. 22° N. The same formation reaches to within $13\frac{1}{2}$° of the Pole. *Ammonites concavus*, from the lower Oolite, was recognized by Haughton, among the fossils brought back by McClintock from Prince Patrick's Island, lat. 77° 10′ N. In Cook's Inlet also, lat. 60° N. several ammonites of Jurassic age, and *Belemnites paxillosus*, a British liassic fossil, were found, while remains of a large ichthyosaur of liassic type were brought from an island in lat. 77° 16′ N. by Sir E. Belcher.[6] Nautili and ammonites of this age have occurred in Spitzbergen, while abundant plant remains of Jurassic age have been forthcoming from Eastern Siberia and Amurland, from Spitzbergen, and from Ando in Norway, just within the Arctic circle.[7] "Liassic fossils were long ago discovered by Lieutenant Anjou of the Russian Navy in the New Siberian Islands; and at Point Wilkie and other localities in the Parry Archipelago, patches of lias with a rich fauna occur. Saurians have been discovered in Spitzbergen in beds referable to the Trias and Lias. Saurian

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[6] Lyell, Principles, i. 218-222.   [7] Wallace, Island Life, 195.

vertebræ were found in the lias beds of Bathurst Island. Jurassic rocks have been examined in Alaska, and Payer and Copeland have found jurassic beds in Eastern Greenland." [8]

The fossils of the Chalk and Greensand are equally conclusive as to the surroundings of life having been warm in those periods. The molluscs, such as Cypræas, Olivas, Tritons Pyrulas, Neritas and Volutas, are essentially tropical shells. "The plants, such as the cycads, Araucarian conifers and ferns, favour," says Lyell, "the hypothesis of a warm climate. Here again we have a large development of gigantic reptiles, such as chelonians, ichthyosaurs and plesiosaurs." [9]

In these beds, again, we find evidence of widespread uniformity of conditions. A very rich Cretaceous flora has been discovered in Greenland, in 70° 33' N. lat., and also in Spitzbergen ; [1] while Cretaceous beds with shells similar to those occurring in Europe occur in beds of the same age within 10 or 12 degrees of the equator, at Pondicherry, Verdachellum and Trinchinopoly. This very widespread fauna and flora proves that the high temperature of the Secondary era pervaded all latitudes, and not only so, it pervaded them apparently continuously without a break. There is no evidence whatever, known to me, that can be derived from the fauna and flora of Secondary times, which points to any period of cold as even possible. There are no shrunken and stunted forms, and no types such as we associate with cold conditions, and no changes evidenced by intercalated beds showing vicissitudes of life.

This being the palæontological evidence, it ought to be conclusive ; but we feel bound to notice some of the instances which have been quoted from the nature of the rocks.

The boulders which Mr. Godwin Austen described in the Triassic beds of Devonshire as showing ice action, were discussed by a very acute geologist, void of prejudices, namely Pengelly, and he argues that they need not have travelled far, and are such as might have been moved by breakers beating against a wasting cliff.

"In regard to the stones in the Rothliegende of North Germany, which I know from personal examination," says Von Cotta, "it is altogether unnecessary to attribute their

[8] Fielden, op. cit. 151. [9] Lyell, op. cit. 213-216.
[1] Island Life, 179-180.

transport to any other agent than water, while scratches have been made on blocks of stone by other agents than ice ; and before we can concede as established, that these evidences prove anything, they need a great deal more careful sifting." [2]

If we turn to the Jurassic beds, much has been made of certain supposed concessions by Professor Judd. It will be well therefore to state his actual views, as printed in the Quarterly Journal of the Geological Society. He says : " It must be confessed that a careful examination of all the facts exhibits some very startling differences between these deposits of Oolitic age and ordinary Boulder days, or indeed any glacial deposits of modern date which have yet been described. . . . Although innumerable opportunities were afforded for observing the surface of the blocks, many of which have evidently not been in the least degree water-worn before being involved in the surrounding and protective matrix, and although I was on the look-out for evidence of glacial markings, through many weeks during which I studied these beds, yet it must be confessed that *in no single instance was I able to detect a clear and indisputable example of any such markings.* . . . The absence of an admixture of foreign blocks from widely different and distant formations, is another feature in which the ' brecciated beds ' differ from those of more modern date, which we now know to have been due to the causes which operated during the glacial epoch." [3] Again he says : " The alternation of the ' brecciated beds ' with the finely laminated and quietly deposited strata, and the confused arrangement of the blocks in the latter, their admixture with trunks of trees, stems of cycads, and other plant remains, seems to indicate that the quiet deposition of the river estuarine beds, was interrupted by the occasional occurrence in the rivers of floods of the most violent character. These appear to have swept angular masses just separated from their parent rock by frosts or landslips, subangular masses, which had been for a time in the course of the streams, and the rounded pebbles of the river-beds, along with trunks of trees torn from their banks, all in wild confusion out to sea, where they were mingled with the sea-derived materials of the shell banks and

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[2] Die Geologie der Gegenwart, 1878, 337.
[3] Op. cit. xxix. 187-193.

shoals. . . . The total absence of glacial polishing and striation from the surfaces of the transported blocks, and the abundance of a splendid flora abounding in cycads, ferns, and large trees on the adjoining land, to say nothing of the characters of the abundant submarine fauna, entirely preclude the idea that these masses were actually heaped together by glaciers which came down to the sea-level. The local character of these blocks, and the absence of the far-travelled boulders, alike indicate that these accumulations could not have been formed by the stranding and melting of icebergs." [4]

In regard to the Jurassic conglomerates of Connecticut, Dr. Wright says: "It is proper to remark that the drift in the lower Connecticut valley would, to a great extent, come from the same region, whether brought by ice or water, and the extent to which the pebbles would be reduced to uniformity and smoothness by attrition depends upon the distance to which they have been rolled, or the length of time which they have been subjected to wave action. From what appears, the evidence is not clear that the fragments from which the pebbles were made may not have originated in the river vicinity, and so their subangular condition need not imply glacial agency in transportation." [5]

In regard to the chalk, I will recall the reader's attention to what I have said in an earlier page as to the tropical life then prevailing. Apart altogether from the possibility of understanding how glacial conditions could exist where these tropical products lived, we should try and realize how, even in the form of icebergs, ice could exist in the seas of the West Indies, if we are also to realize the perverse credulity of some geologists.

When we turn to the stones which have been found in the chalk, they are rounded and smoothed, and of very moderate size, sporadic, and free from striæ. They differ, in fact, in every particular from true glacier stones. They may have been carried by seaweed, or entangled in the roots of trees, as the stone mentioned by Lyell, as big as a man's head, which was carried 600 miles to Keeling Island, a small ring of coral in the Indian Ocean.[6] Mr. J. Gunn, referring to the boulder of granite found by Godwin Austen in the chalk, says,

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[4] Id. 195.   [5] The Ice Age in North America, 436-7.
[6] Principles, i. 217.

" It exhibits no more than the washing of the land by floods or inroads of the sea. . . . Such masses are rounded, and afford no proof of ice action." [7]

Let us now turn to Tertiary times, where the evidence is even more inconclusive. This very inconclusiveness has led Croll and others to enlarge at this stage on a convenient argument, the alleged imperfection of the geological record. In regard to this favourite appeal of the ultra-glacialists, Wallace says : " It will not help us here (*i.e.* to explain the absence of traces of glacial action in the Tertiary beds), because the series of tertiary deposits is unusually complete, and we must suppose some destructive agency to have selected all the intercalated glacial beds, and to have so completely made away with them that not a fragment remains, while preserving all or almost all the interglacial beds, and to have acted thus capriciously, not in one limited area only, but over the whole northern hemisphere, with the local exceptions on the flanks of great mountain chains already referred to." [8]   Again the same writer says : " Admitting the argument that denudation has destroyed a good deal of evidence, there is nevertheless one kind of evidence that we ought to find, because it is widespread and practically indestructible." This consists, he urges, in the rock débris dropped by icebergs, and the mud and imbedded stones formed by moraines. "We cannot doubt," he continues, " that throughout the greater part of the duration of a glacial epoch, the seas adjacent to the glaciated country would receive continual deposits of large rocks, rock fragments and gravel, similar to the material of modern and ancient moraines, and analogous to the drift and the numerous travelled blocks which the ice has undoubtedly scattered broadcast over every glaciated country ; and these rocks and boulders would be imbedded in whatever deposits were then forming, either from the matter carried down by rivers or from the mud ground off the rocks and carried out to sea by the glaciers themselves. Moreover, as icebergs float far beyond the limits of the countries which give them birth, these ice-borne materials would be largely imbedded in deposits forming from the denudation of countries which had never been glaciated, or from which the ice had already disappeared." If periods

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[7] Geol. Mag. ser. III. i. 74.   [8] Island Life, 175.

of high eccentricity always produce glacial epochs, or if glacial epochs are recurrent phenomena, we ought to find numerous and extensive boulder and rock strewn deposits. On the same subject Dr. Wright has the following graphic sentences : " It does not seem possible that a glaciated area so extensive as is that of North America, and so deeply covered with glacial débris, could be so completely removed by ordinary denuding agencies that no more signs of it should appear than are found of such phenomena in the earlier geological epochs; for the till or ground-moraine is not readily removed by the action of water, even when subjected to the shore waves of the ocean. The boulders which are washed out of it form a protecting barricade around the base of the deposit, so that islands like those in Boston harbour, composed wholly of till, are as nearly proof against the waves as are those of ordinary rocks. If there were in progress a subsidence of the glaciated area of North America, instead of having the waves wash the glaciated surfaces away gradually from the edges inward, we should find merely an encroachment made here and there upon the border during a portion of the subsidence, until finally, when the waters covered the whole, all but a very thin stratum of the upper portion would be protected from further disturbance. Especially must the till remain in the innumerable buried channels of the glaciated region, and over the extensive protected northern slopes. It is thus difficult to conceive how there should ever be any such complete removal of the ground-moraine from the immense glaciated area of North America as Mr. Croll supposes to have occurred several times over in preceding glacial epochs." [9]

These statements are incontrovertible ; let us apply them to the Tertiary beds. " The whole of the Tertiary deposits," says Wallace, " in the north temperate and arctic zones, should exhibit constantly alternating boulder and rock bearing beds, or coarse rock-strewn gravels analogous to our existing glacial drift, and with some corresponding change of organic remains." [1] But when we proceed to examine the Tertiary deposits of Europe, save and except the very local Miocene beds of the Superga and the Eocene beds known as Flysch, and especially those of our own country, not only is such evidence

[9] The Ice Age in North America, 432, 433.   [1] Island Life, 171.

completely wanting, but the facts are of so definite a character as to satisfy most geologists that it can never have existed; and the same may be said of temperate North America and of the arctic regions generally." [2]

If we examine the Tertiary beds both in Europe and North America in regard to their fossil contents, we shall find no evidence of cold periods at all. "They indicate, as Wallace says, throughout warm or temperate climates, except those of the more recent Pliocene deposits which merge into the earlier glacial beds. The Miocene deposits of Central and Southern Europe, for example, contain marine shells of some genera now only found farther south, while the fossil plants often resemble those of Madeira and the Southern States of North America. Large reptiles too abounded, and man-like apes lived in the South of France and in Germany. . . . The Eocene period is even more characteristically tropical in its flora and fauna, since palms and Cycadaceæ, turtles, snakes, and crocodiles then inhabited England." [3]

Lyell admits, in regard to the Eocene beds of the Alps, that it has been particularly remarked by M. Desor, that the strata most nearly associated with the flysch are rich in echinoderms of the Spatangas family, which have a decidedly tropical aspect. These strata also contain nummulites and genera of fossil plants and animals characteristic of a warm climate. The flysch itself contains neither shells nor other organic remains, and this, as in the case of the Miocene beds already referred to, has been quoted as a proof of their having been deposited under glacial conditions. In regard to this argument Lyell says, "This negative character is too common in strata of every age to be of much value, except in connection with other proofs of intense cold. Nor must we disguise from ourselves the fact, that in the seas of polar regions where icebergs abound at present there is by no means any dearth of animal life." [4]

Mr. Wallace also refers to the enormous series of deposits constituting the English Eocene beds many thousands of feet thick, consisting wholly of alternations of clays, sands, muds, shells or limestone, with a few beds of pebbles or conglomerate, not one of the whole series containing irregular blocks

[2] Id. 173.   [3] Id. 171-2.   [4] Principles, i. 210.

of foreign materials, boulders or gravels, such as are essentially characteristic of a glacial epoch, and he argues that we must be forced to the conclusion that no general glacial epochs could have occurred during their formation.[5]

In a paper on the climate controversy by Mr. Searles V. Wood, junior, a deservedly high authority on the Tertiary beds, we read : " The Eocene formation is complete in England, and is exposed in continuous section along the north coast of the Isle of Wight from its base to its junction with the Oligocene, (or Lower Miocene according to some), and along the northern coast of Kent from its base to the Lower Bagshot Sand. It has been intersected by railway and other cuttings in all directions and at all horizons, and pierced by wells innumerable ; while from its strata in England, France, and Belgium, the most extensive collections of organic remains have been made of any formation yet explored, and from nearly all its horizons, for at one place or another in these three countries nearly every horizon may be said to have yielded fossils of some kind. These fossils, however, whether they be the remains of a flora such as that of Sheppey, or of a vertebrate fauna containing the crocodile and alligator such as is yielded by beds indicative of terrestrial conditions, or of a molluscan assemblage such as is present in marine or fluvio-marine beds of the formation, are of unmistakably tropical or sub-tropical character throughout ; and no trace whatever has appeared of the intercalations indicative of more than one period of 10,500 years' glaciation. Nor can it be urged that the glacial epochs of the Eocene in England were intervals of dry land, and so have left no evidence of their existence behind them, because a large part of the continuous sequence of Eocene deposits in this country consists of alternations of fluviatile, fluvio-marine, and purely marine strata ; so that it seems impossible that during the accumulation of the Eocene formation in England a glacial period could have occurred without its evidences being abundantly apparent. The Oligocene of North Germany and Belgium, and the Miocene of those countries and of France, have also afforded a rich molluscan fauna, which, like that of the Eocene, has as yet presented no indication of anything to interfere with its uniformly sub-tropical character." [6]

[5] Island Life, 174-5.   [6] Geol. Mag. 1876, 392.

It can hardly be contended that the mollusca adapted themselves to a refrigerated sea, and again to a warm one, without the facies of the fauna being changed, because we find during the succeeding period, the Pliocene, the molluscan fauna gradually changing by the disappearance of the tropical and sub-tropical genera, and by the incoming of species which now generally inhabit Arctic and Boreal seas, thus indicating the gradual approach of those climatic conditions which culminated in the glacial period." [1]

Mr. Wood, turning to the flysch of Switzerland and the beds of the Superga, goes on to urge the improbability that at two distinct epochs during this uninterrupted prevalence of tropical and sub-tropical conditions in England and the adjoining countries, glaciers descended to the sea several degrees farther south, and argues that neither lofty land nor ocean currents could have modified the climate of Switzerland or North Italy to such a degree as to allow glaciers to descend to the sea there, while countries lying several degrees of latitude nearer the pole were enjoying a tropical climate. [2]

Turning to the Miocene conglomerates, Lyell tells us how they "are covered by strata containing shells of the Upper Miocene formation, and rest on Lower Miocene strata, for the most part of fresh-water origin. The fauna and flora both of the underlying and overlying rocks having the same sub-tropical character as that of other beds of Miocene date in Switzerland and in Central Europe generally." [3] The beds have themselves yielded no organic remains, which fact has been referred to by Croll and others as evidence of their glacial origin, but this seems to me inconsequent. Lyell himself says of this argument : " Such a hypothesis is not very satisfactory, because the thickness attained by the conglomerate in some parts of Piedmont is very great, far exceeding that seen in the neighbourhood of Turin. We must conclude, therefore, that its accumulation occupied a great lapse of time, and if so, it is difficult to understand why there are no organic remains in it ; for, although the temporary influx of a cold current might well be supposed to annihilate a fauna fitted for a warmer sea, yet the long continuance of such a current would naturally fit the region for species such as thrive in the seas of colder

[1] Geol. Mag. 392-3.   [2] Id. 393.   [3] Principles, i. 208.

latitudes."[4] If we turn from the organic remains to the petrology of the Lower Tertiaries, we shall come to the same conclusion.

In regard to the flysch, the deposit itself does not bear the impress of a glacial deposit. The blocks bear no traces of striation, like so-called glacial erratics do ; while instead of being heterogeneous and without any arrangement, like till and other similar beds and like true moraines, it is character-ized in a large part by regular stratification and fine laminæ. There is only one feature about its boulders, beside their size and angularity, which recalls the erratics elsewhere, namely, that they have travelled some distance; but about this also there is some doubt.

Lyell says, " The nearest spots where any similar serpentine and greenstone occur are about twenty miles to the westward, but there has been so much subsidence of the country during the Miocene period, so much subsequent deposition of over-lying miocene, pliocene, and alluvial deposits, and such changes in physical geography, that we cannot form any probable conjecture as to the proximity or distance of the spots from which the blocks may have come."[5]

The question then narrows itself to this : When we find a number of big blocks in a stratum, and every kind of evidence converges against their being ice borne, are we, because of the exigencies of a fanciful theory that is at issue with astronomy and meteorology, to attribute their transport to ice, simply because they are large angular stones? Assuredly we can do nothing of the kind.

Nordenskiold says very aptly, 'I am convinced that sharp-cornered stones, or stone fragments inclosed in considerable quantity in a bed of sand or clay (so-called Krosstensgrus), by no means afford certain evidence that it is of glacial origin. In this case the mistake is fallen into, as in many other geological questions, of concluding, because on one occasion a cause A produced an effect B, that the effect B is always produced by the cause A. Gravel with sharp-cornered stone fragments is always found, at least in countries where frost tends to break up the rock surface, at the foot of steep mountain-sides clear of vegetation, and on Spitzbergen also

[4] Principles i. 208.　　　　　　　　　　[5] Id. 208.

on the flats, which in many places surround the foot of a mountain, where the ground consists of such a gravel bed formed by frost, which is increased by every spring flood, and then being drenched with water, is exceedingly difficult to cross, and very destructive for the shoes." [6]

In regard to these same beds, Professor Judd writes : "It has been argued from the existence of large blocks and the poverty of the fauna in certain sub-Alpine deposits, that 'glacial epochs,' or a general refrigeration of climate in the Northern Hemisphere, occurred both in the Eocene and the Miocene periods. We shall not stay to point out how cautiously the presence of merely transported blocks—not exhibiting any glacial striation—should be accepted as evidence of glacial action, in the face of the facts which have been published by Mr. Drew concerning the composition of the 'fans' of the Himalaya." [7]

He also argues that even if there was an extension of glacial action in the Alps during the Eocene and Miocene periods, it must have been of a very local character. In proof of this he quotes the conclusions of Mr. Searles Wood already referred to, and continues : "Not only are we met by the fact of the absence of even the smallest trace of the physical action of glaciers at this period elsewhere, but as every student of the British, French, and Belgian Tertiaries is well aware, there is not a particle of evidence in favour of such dwarfing of certain species, accompanied by the migration and extinction of others, which could not fail to accompany the extension of the supposed polar ice caps during parts of those periods.

"Again, we have in the case of the Miocene, in the Vienna basin, a complete series of deposits formed in the same latitude with the Alps, which exhibit in the characters of their faunas clear evidence of a gradual passage from tropical, through sub-tropical, to the temperate conditions of the adjoining seas, but no trace whatever of any such interruption as could not fail to have been produced by a period of excessive cold. . . . Nor is the evidence against the universality of glacial periods wholly derived from Palæontology. The general absence of recent marks of glacial action

[6] Geol. Mag. 1876, 266.　　　　　　　　　[7] Id. 534.

in Eastern Europe is well known, and the series of changes which have been so well traced and described by Professor Szabó, as occurring in those districts, seems to leave no room for those periodical extensions of ice caps with which some authors in this country have amused themselves and their readers." [8]

Dawkins says, speaking of the Miocene blocks found on the Superga: "It seems to me that these blocks do not prove a severe climate in any place, except where the ice in question has been produced, which may have been on the tops of lofty mountains, like those of the Andes, which send glaciers down to the sea in Eyre Sound, Patagonia, in the latitude of Paris. They tell us no more of the Miocene climate of Europe than the glaciers at present in New Zealand tell us of a climate which is sufficiently mild to allow the growth of tree ferns and Areka palms. It is impossible that a great climatal change could have taken place in the Miocene age, affecting Europe generally, without leaving its mark in the flora and the fauna. One severe winter would have destroyed the evergreen forests and the exotic plants, and animals would disappear and be replaced by others capable of flourishing under the new conditions. The blocks of stone may have been carried down by glaciers from the Alpine chain, then lifted high up above the sea into the icy temperature which is to be met with everywhere on the earth at great altitudes. They may be referred to that glacial climate which is above our head even at the equator, rather than to glacial conditions extending down to the sea level in Italy, in a period when the climate of middle and northern Europe was warmer than it is now." [9]

In this view Professor Judd concurs. Thus he says: "In the case of the Alps, I know of no glacial phenomena which are not capable of being explained, like those of New Zealand, by a great extension of the area of the tracts above the snow line, which would collect more ample supplies for the glaciers protruded into the surrounding plains." [1]

I will conclude my survey of this part of my subject by

[8] Geol. Mag. 1876, 535.   [9] Early Man in Britain, 65-66.
[1] Geol. Mag. 1876, 537.

devoting a few paragraphs to the Arctic regions, where the evidence of intercalated mild and severe climates ought to be most conspicuous, and where we shall find it to be even more inconclusive.

In a paper on the Geology of Spitzbergen, Nordenskiold says: "All the fossils found in the foregoing strata, show that Spitzbergen, during former geological ages, enjoyed a magnificent climate, which indeed was somewhat colder during the Miocene period, but was still favourable for an extraordinarily abundant vegetation, much more luxuriant than that which now occurs even in the southern part of Scandinavia; and I have in these strata sought in vain for any sign that, as some geologists have of late endeavoured to render probable, these favourable climatic conditions have been broken off by intervals of ancient glacial periods. The profiles I have had the opportunity to examine during my various Spitzbergen expeditions would certainly, if laid down in a line, occupy an extent of *a thousand English miles;* and if any former glacial period had existed in this region, there ought to have been some trace to be observed of erratic blocks or other formations which distinguish glacial action. But this has not been the case. In the strata, whose length I have reckoned above, I have not found a single fragment of a foreign rock so large as a child's head. The round nodules in the Triassic and Jurassic strata are clearly concretionary formations, which contain the same fossils as the surrounding strata."[2] Again he adds: "In the deep ravines formed by streams, which in N.W. Greenland cut through first the sands formed during the glacial period, and lower down the Miocene beds, we have an excellent opportunity of seeing the difference between them, which is very noticeable, inasmuch as erratic blocks are never absent in the former but always in the latter."[3]

In an elaborate paper published in the *Geological Magazine*, entitled "Former Climates of the Polar Regions," the same writer says, *inter alia:* "From what has been already stated, it appears that the animal and vegetable relics found in the polar regions, imbedded in strata deposited in widely

[2] Geol. Mag. 1876, 266.   [3] Id., note.

separated geological eras, uniformly testify that a warm
climate has in former times prevailed over the whole globe.
From palæontological science no support can be obtained
for the assumption of a periodical alternation of warm and
cold climates on the surface of the earth." [4]   Again he says :
" A careful investigation of the structure of the different
sedimentary strata leads to the same result.   We are now
very well acquainted with the origin and nature of the
various strata, the substance of which has been supplied by
the destructive operation of glaciers on the surrounding
and subjacent masses, and we can point out certain marks
by which these strata may be distinguished from other
non-glacial deposits.   In these last one very rarely meets
with any large stone boulders, which have fallen from some
neighbouring cliff and been imbedded in sand or clay, either
directly, and if so, close to the place where originally found,
or else after having in the spring been moved a greater
or less distance by river ice.   In glacial formation, on the
contrary, as one may gather from the study of the strata
in Scandinavia that belong to the glacial period, erratic
blocks transported on icebergs to far-distant regions play
an important part.   If a climate similar to that which now
prevails in the Arctic regions has several times during various
geological eras existed in the neighbourhood of the Pole,
one has reason to expect that sandstones enclosing large
boulders should often be met with in these tracts.   But
this is by no means the case, though such formations,
if they exist on a large scale, could hardly escape obser-
vation.

" The character of the coasts in the Arctic regions is
especially favourable to geological investigations.   While
the valleys are for the most part filled with ice, the sides
of the mountains in summer, even in the 80th degree
of latitude, and to a height of 1000 or 1500 feet above
the level of the sea, are almost wholly free from snow.
Nor are the rocks covered with any amount of vegetation
worth mentioning, and, moreover, the sides of the moun-
tains on the shore itself frequently present perpendicular
sections, which everywhere expose their bare surfaces to the

[4] Op. cit. vol. xii. 531.

investigator. The knowledge of a mountain's geognostic character, at which one in more southerly countries can only arrive after long and laborious researches, removal of soil and the like, is here gained almost at the first glance; we have never seen in Spitzbergen, or in Greenland, in these sections often many miles in length, and including, one may say, all formations, from the Silurian to the Tertiary, any boulders even as large as a child's head. No strata containing boulders are to be found in the Polar tracts previously to the middle of the Tertiary period.

" Since, then, both an examination of the geognostic condition, and an investigation of the past flora and fauna of the polar lands, show no signs of a glacial era having existed in those parts before the termination of the Miocene period, we are fully justified in rejecting, on the evidence of a clear observation, the hypothesis founded on purely theoretical speculations, which assumes the many times repeated alternation of warm and glacial climates, between the present time and the earliest geological ages." [5]

After referring to similar facts, Wallace says : " If we take these facts as really representing the flora of the period, we learn that whenever plant remains have been discovered in the Arctic regions, either in Tertiary or Cretaceous deposits, they show that the climate was one capable of supporting a rich vegetation of trees, shrubs and herbaceous plants, similar in general character to that which prevailed in the temperate zone at the same periods, but showing the influence of a less congenial climate. These deposits belong to at least four distinct geological horizons, and have been found widely scattered within the Arctic circle, yet nowhere has any proof been obtained of intercalated cold periods, such as would be indicated by the remains of a stunted vegetation, or a molluscan fauna similar to that which now prevails there." [6] Again he says : " It is quite impossible to ignore or evade the force of the testimony as to the continuous warm climate of the north temperate and polar zones throughout Tertiary times. The evidence extends over a vast area, both in space and time, it is derived from the work of the most competent living geologists, and it is absolutely consistent in its general

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[5] Op. cit. vol. xii. 531-2.   [6] Island Life, 180-1.

tendency. We have in the Lower Cretaceous period an almost tropical climate in France and England, a somewhat lower temperature in the United States, and a mild insular climate in the Arctic regions. In each successive period the climate becomes somewhat less tropical, but down to the Upper Miocene it remains warm temperate in Central Europe, and cold temperate within the polar area, with not a trace of any intervening periods of arctic cold." [7]

"If we now look at the whole series of geological facts," says the same writer, "as to the animal and vegetable productions of the Arctic regions in past ages, it is certainly difficult to avoid the conclusion that they indicate a climate of a uniformly temperate or warm character. Whether in Miocene, Upper or Lower Cretaceous, Jurassic, Triassic, Carboniferous or Silurian times, and in all the numerous localities extending over more than half the polar regions, we find one uniform climatic aspect in the fossils. This is quite inconsistent with the theory of alternate cold and mild epochs during phases of high eccentricity and persistent cold epochs, when the eccentricity was as low as it is now or lower. . . . Why then should the fauna and flora of the cold epochs *never* be preserved? Mollusca and many other forms of life are abundant in the Arctic seas, and there is often a luxuriant dwarf woody vegetation on the land, yet in no one case has a single example of such a fauna or flora been discovered of a date anterior to the last glacial epoch. And this argument is very much strengthened when we remember that an exactly analogous series of facts is found over all the temperate zones. Everywhere we have abundant floras and faunas indicating warmer conditions than such as now prevail, but never in a single instance one which as clearly indicates colder conditions. The fact that drift with Arctic shells was deposited during the last glacial epoch, as well as gravels and crag with the remains of Arctic animals and plants, shows us that there is nothing to prevent such deposits being formed in cold as well as in warm periods ; and it is quite impossible to believe that in every place and at all epochs all records of the former have been destroyed, while in a considerable number of in-stances those of the latter have been preserved. When to this

[7] Island Life, 182.

uniform testimony of the palæontological evidence we add the equally uniform absence of any indication of the ice-borne rocks, boulders, and drift, which are the constant and necessary accompaniment of every period of glaciation, and which must inevitably pervade all the marine deposits formed over a wide area so long as the state of glaciation continues, we are driven to the conclusion that the last glacial epoch of the northern hemisphere was exceptional, and was not preceded by numerous similar glacial epochs throughout Tertiary and Secondary time." [8]

I will conclude this part of the subject with some opinions of geologists of weight with whom I agree. Sir Charles Lyell says : "If the sketch we have given in the tenth and eleventh chapters, of the former states of climate revealed to us by palæontological research, be an approximation to the truth, it will be at once seen that glacial periods have not been perpetually recurring in the northern temperate zone, as they ought to have done were a large eccentricity alone sufficient, apart from the co-operation of all other causes, to intensify the cold of high latitudes. It was shown that the flora and fauna do not exhibit signs of violent revolutions from hot to cold and from cold to hot periods. On the contrary, the continuity of forces, particularly in the class of reptiles, from the Carboniferous to the Cretaceous period, is opposed to the intercalation of glacial epochs corresponding in importance to that of post-Pliocene date. During the Carboniferous period there must have been a long suspension, in the temperate latitudes of the northern hemisphere, of cold such as we now experience, an equable climate must have endured for a lapse of centuries, sufficient to allow several giant cycles of eccentricity to be gone through ; but we do not find in strata of that age, 15,000 feet thick in Nova Scotia, any proofs of such intercalated glacial epochs. The peculiar vegetation of the coal was persistent throughout the greater part of the ages required for the deposition of so great a thickness of sediment, in which one forest after another was buried on the spot where it had grown." [9]

Professor Le Conte says : "Of the recurrence of many glacial epochs in the history of the earth there is as yet no

[8] Island Life, 195-6.   [9] Principles, Ed. 1867, 299.

reliable evidence, but much evidence to the contrary. It is true that what seem to be glacial drifts with scored boulders, etc., have been found on several geological horizons; but these are usually in the vicinity of lofty mountains, and are probably therefore evidence of local glaciation, not of a glacial period. On the other hand, all the evidence derived from fossils plainly indicates warm climates, even in polar regions, during all geological periods until the Quaternary. The evidence at present, therefore, is overwhelmingly in favour of the uniqueness of the glacial epoch. This fact is the great objection to Croll's theory." [1]

With this I completely agree. Let us now discuss the further question, whether the so-called glacial epoch has itself been broken up into a series of stages by a number of mild intervals. When we turn from the older beds to those representing the last great geological horizon, we are met by increasing instead of diminishing difficulties, and both the facts and their interpretation present us with most stubborn problems. Mr. Croll, in formulating his well-known theory, was specific enough and bold enough : " What then," he says, " is the date of the glacial period ? It is quite clear that if the glacial period resulted from a high state of eccentricity, it must be referred either to the period extending from about 980,000 to about 720,000 years ago, or else to the period extending from 240,000 years and reaching to about 80,000 years ago. As the former period was of greater duration than the latter, and the eccentricity also attained to a higher value, I was at first disposed to refer the glacial epoch proper (the time of the till and boulder clay) to the former period ; and the latter period, I was inclined to believe, must have corresponded to the time of local glaciers towards the close of the glacial epoch, the evidence for which (moraines) is to be found in almost every one of our Highland glens. On this point I consulted several eminent geologists, and they all agreed in referring the glacial epoch to the former period." He goes on to tell us that he himself was convinced after careful inquiry that it must be referred to the latter, and he proceeds in the gravest way to apologize for the comparatively small demands he makes

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[1] Elements of Geology, p. 577.

upon time in placing the close of the glacial epoch no farther back than 80,000 years ago.

His argument of course involves that before the glacial period proper, and between 980,000 and 720,000 years ago, there must have been vicissitudes of climate of the most extreme character; far more extreme, in fact, than during the glacial period proper. Of this, as we have seen, there is literally no evidence in the geological record. Let us, however, pass on, limiting ourselves to the glacial period proper. Croll's position is that, during the 160,000 years which it lasted, there was an alternation of severe and mild climates in each hemisphere every 10,000 or 12,000 years.

Suppose we allow our imagination to run riot, and that we concede his portentous chronology as even possible, what evidence do the so-called glacial beds afford of these vicissitudes? I may say the evidence is virtually wanting to support such a conclusion. Of the many alternating warm and cold periods required by his theory, Croll produces evidence of no more than two or three, and this, as we shall see presently, of a very unsatisfactory character. This he realizes, and in order to explain the hiatus he appeals again to the imperfection of the geological record. He nowhere explains why in the case of the glacial beds we should have a reversal of the rule which holds good in all the other beds, and have so few traces of the plants and animals that thrive under warm conditions and so much evidence dating from the colder periods. Surely the molars of mammoths, the leg-bones of oxen and horses, are not more fragile than Arctic shells and the leaves of Arctic plants.

Other geologists have not demanded quite so much as Dr. Croll. They have been content to appeal to one, two, or three glacial periods, and this not so much on *a priori* grounds, but because they believe the evidence of the beds themselves demands this conclusion. Now the first and most remarkable thing is, that the interglacialists are not agreed among themselves as to the number of ice periods, and yet one would suppose that no evidence ought to be more easily read and more conclusive, for if the alternating periods represent a duration of 10,000 or 12,000 years, surely their traces ought to be forthcoming somewhere. We will concede to Mr. Geikie and

others the convenient but unsatisfactory explanation of the com-
plete absence of any traces of interglacial beds in the mountains
of Norway, of Switzerland, and the north of Scotland, at high
levels, namely that being highlands there were never great
traces of warm periods to be found there, and that the rapid
denudation that takes place has long removed whatever there
were; but this argument cannot apply to the low country of
South Holland and Belgium, and the valleys of South Britain
and France and of the Southern States of America. There the
ice plough has not furrowed the ground, and the succession
of beds ought to be intact; yet there, so far as I know, there
is absolutely no trace of alternating climates at all. This in
itself seems to me to be a fatal objection.

Let us pass on, however. Scarce and sporadic as the so-
called interglacial beds are, they are only found in that zone
where great boulders or till or other forms of drift are con-
spicuous. They may be divided into two classes, inorganic
and organic. In regard to the inorganic facts, which
consist chiefly in the intercalation of beds of more or less
stratified sand and loam, between and among beds of un-
stratified clay containing boulders, I altogether refuse to
entertain them as evidence of a succession of climates. Their
testimony is most unsafe and dubious. We have not yet found
a way by which we can give a rational explanation of the
true succession of so-called glacial deposits in two adjoining
counties, much less in two larger geographical areas. No-
where do we meet with clearer proof of tumultuous and
disorderly deposit than in these beds of boulder clay without
stratification, and in these local beds of intercalated sand,
with their lines of deposit twisted into every conceivable
form of curve and eddy. In many places we have no sands
at all, in many very large areas we have no boulder clay
at all; and yet the accidental occurrence of pockets of
stratified sands or loam, sporadically distributed, is held to
prove an interglacial climate.

Let us turn then to the only really satisfactory tests of
climate, namely the remains of the plants and animals. In
regard to these I would make two preliminary remarks. In
some places the remains of animals and plants which are
generally associated with a warm climate, such for instance

as the hyæna, the lion, the fig, and the canary laurel, have been found mixed together with animals and plants generally associated with the cold climate, such as the musk sheep, the reindeer, and the polar willow, and this juxtaposition of the relics of a warm and cold period has been pointed to as a singular proof of interglacial climates. Mr. James Geikie, in his address at Newcastle, especially enlarged on this view, which some of us thought had become obsolete. How can such an argument be sustained when all over France, Germany, and South Britain we not only find reindeer bones mixed with those of the hyæna, but, as Professor Dawkins has shown, gnawed by hyænas; when we meet with the leaves of the arctic willow mixed with those of the canary laurel and the fig, to mention only two facts out of a great many which may be found collected in " The Mammoth and the Flood."

Secondly, it is a singular fact that, where more than one interglacial bed has been supposed to have left traces of a warm climate, we should have these successive warm or cold climates marked by precisely the same animals and plants. D'Archiac long ago urged the extreme improbability, if not impossibility, of such a thing happening. If we limit ourselves to much more recent times, we shall find no support for this view. In the bogs of Denmark and the north of Sweden we have a succession of beds, beginning with those containing stone axes. In these bogs we find the remains of a succession of forests, but not of the same trees. We begin with pines, then we have oaks, and lastly beeches. The same thing occurs in the bogs of Holland. Erdmann tells us that after a fire in Siberia which has burnt down a pine forest, there grow up not pines but birches and poplars, and the same observation has been made in America. If this be so where the conditions are continuous and where the seeds of the old forest are scattered about, how strange would it be to find that after an interval of over 10,000 years we should have precisely the same trees growing on the same ground, not in rich soil, but on the surface of the cold barren clays deposited by the glacier. On this latter point I should like to quote a sentence from an American geologist.

Mr. McGee, speaking of the difficulty presented by the

immense time required to re-fertilize a district once occu-
pied by ice, so as to produce a luxuriant flora like that
of the North American forests, says: "We know that
crude soils such as fresh glacial clays from some yards
beneath the surface, are not adapted to the support of a
luxuriant vegetation. Thistles, yuccas, cacti, and other hardy
plants may spread over barren clays, sterile sands, and
tufaceous wastes, and are sometimes planted with the object
of reclaiming such areas; but even when other germs are
not lacking, it is only after considerable periods that a richer
flora supersedes these plant pioneers. After the retreat of
a glacier, however, there would be a dearth of seeds and
germs, and their spread over the glaciated wastes would be
slow. This is substantiated by the observations of Professor
Alphonse de Candolle in the Alps, and of Professor Blyth
of Christiania in Scandinavia, who find that ' of the valleys
laid bare at the glacial period, those whose glaciers retreated
first, present a richer and more varied vegetation than those
which remained a long time covered with ice.' Dr. Hayden
also observed an analogous phenomenon during the season
of 1877. The western slope of the Rocky Mountains ex-
hibits abundant traces of glaciation evidently quite recent,
geologically speaking, though it may well be doubted
whether the glaciers have existed within many centuries, yet
scarcely any vegetation has sprung up on the light glacial
soil." [2]

If this be the case with plants, how much more so with
animals, which for the most part live on the plants. The fauna
of a country is always being modified, and in continental
areas is being changed by the slow importation and spread
of new forms, and we cannot realize how there should be a
succession of periods at long intervals marked by the same
fauna. It is contrary to all the evidence of palæontology.

Again, to limit ourselves to the animals. So far as the evi-
dence is available, in those European districts where it can be
tested best, the remains of the so-called Pleistocene fauna, when
in situ, is overlaid by the glacial beds. Of course, tree-trunks,
bones, and antlers often occur in the boulder clay itself, but
when they do so it is as boulders, and not because they repre-

sent an old land-surface. The Pleistocene fauna, so far as I know, came to an end with the so-called glacial age. On this I shall have more to say on another occasion. Here I will only affirm that this fact, if it be a fact, is conclusive against supposed finds of mammals' bones in the drift being accepted as evidence of interglacial mild climates.

Having stated these *a priori* grounds, let us examine the evidence in greater detail.

In regard to that from Switzerland, which has been so frequently quoted, it is in the first place singular that it should have failed to convince some of the most experienced geologists who have treated of glacial phenomena. This has been remarked by Penck, who refers for evidence of the statement to A. Favre, Lory, Falsan and Chantre, De Mortillet, Gastaldi, Desor, Charles Grad, and E. von Mojsisovics. They all argue that, granting the lignite-beds lie between two actual glacial deposits, the evidence does not point to any intervening mild climate, but only to an oscillation in the movement of the ice, such as we habitually find in existing glaciers, and that the plants that have been found in the beds are such as still grow in proximity to glaciers. Penck, himself a strong advocate of more than one glacial age, nevertheless points out how modern glaciers sometimes advance into wooded regions —that wheat grows not far from the Aar glacier—that there is a corn-field within 300 metres of Buervrâ in Norway, and that fruit grows in the Hardangerfiord within three kilometres of the inland ice.[3]

Apart from the concurrence of testimony just referred to, there is the further fact that the evidence relied upon is itself most suspicious. That the coal-beds at Dürnten and Utznach are overlaid by drift I have no doubt ; that they are also under-laid by it seems to be most doubtful, if not actually unfounded. Heer himself, upon whom the conclusion is generally placed, speaks in the most guarded possible way. Thus he says: " In Eastern Switzerland, until recently, the upper erratic formation only was known, which overlies the stratified pebble-beds. At Utznach, in a section on the road to Gausen, now again covered up, Professor A. Escher von der Linth saw quite distinctly the direct superposition of the lignite forma-

[3] Vergletscherung der Deutschen Alpen, 221-222.

tion upon the Miocene beds; so that, at least at this point, there were no traces of erratic blocks to be seen between the Miocene beds and the lignites. At Dürnten, also, the clays under the lignite formation only contain such stones as may have been derived from the conglomerates of the surrounding hills. The hypothesis of two glacial periods does not, therefore, seem to be supported by the facts observed in Eastern Switzerland."

The same views are maintained by Sir Charles Lyell. He visited the beds at Utznach in company with M. Escher von der Linth, and those at Dürnten with Professor Heer and M. Marcou. " The beds in both places," he says, " are of the same age. At Utznach the lignite beds rest directly, but unconformably on highly inclined and sometimes vertical Miocene molasse, and are covered with gravels and these again with erratic blocks, and he distinctly says that the evidence is wanting at Utznach to establish their interglacial character.[4]

In regard to the deposits at Dürnten, M. Charles Grad, who examined them with great care, found the underlying beds to consist of light coloured clays or marls, such as occur in freshwater beds in lakes and marshes. These marls contain numerous rolled pebbles, but M. Grad says, " Je n'y ai pas remarqué de stries glaciaires." [5]

This is assuredly a very indifferent base upon which to erect the gigantic inferences that have been built upon it, and it seems to me that in so far as the principal lignite deposits of Dürnten and Utznach are concerned, the evidence breaks down completely. A similar bed occurs at Wetzikon, near Dürnten, in which the plants are exactly similar, and which is doubtless of the same age. There, we are told, Alpine blocks have been found underneath the lignite, but the evidence about it is unsatisfactory. In the first place, Heer speaks of this lignite bed as of small extent. In the next place, it is unfortunate that this particular and most important section should have been entirely covered up and be no longer available for study. It is also unfortunate that it should have been an artificial and not a natural section. Mortillet has called attention to the very unsatisfactory nature of artificial sections made by workmen, and not under the eye of scientific men,

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[4] Lyell, Antiquity of Man, 367-369.
[5] Recherches sur les charbons feuilletés interglaciaires, 1877, p. 4.

and speaking of this very one, he tells us how easy it is in deep pits cut through soft strata to have a slipping of portions of the beds. M. Charles Grad, again, in discussing it, says that it was much disturbed. He had himself noticed a streak of gravel traversing the lignite; in another place the same lignite was penetrated by a wedge of clay. In some places it rested in small pockets on the gravel and the clay; in others it was standing vertically and merely covered by vegetable soil. These circumstances point, he says, to slipping or crushing of the lignite beds after they were formed.[6] This description surely destroys the whole value of the section as a crucial and critical test, and, as Mortillet suggests, it points to the supposed subjacent glacial beds having been accidentally and not naturally so placed.[7]

The beds at Zurich are, I believe, distinctly pre-glacial in the sense in which the word is generally used, that is, they are overlaid and not underlaid by drift. The fauna again points to this, since the elephant and rhinoceros found in them are not the mammoth and the *R. Antiquitatis*, but the *E. Antiquus* and *R. Leptorhinus*, the same beasts found in the Forest Bed, which is an earlier horizon than that where the mammoth occurs, and *a fortiori* one more unlikely to occur among true glacial beds. Neumayr, in his " Erdgeschichte," says that similar remains occur in the beds at St. Jacob on the Birs, near Basle, in the Algau near Sonthofen, at Chambery and Sonnaz in Savoy, and at Leffe and Val Gandino in Upper Italy, in none of which places is the bed in which they occur intercalated between glacial beds.[8]

The strength of this latter evidence has been very much overlooked. Thus, at Rapperschwyll, in the canton of St. Gallen, Escher von der Linth gives a section as follows :—

1. Erratic blocks and glacial débris.

2. Rolled pebbles from the size of an apple to that of a man's head.

3. Blue and yellow clays.

4. Bituminous bed (lignite, containing pine, junipér, and birch) mixed with sand and clay.

5. A piece of a mammoth's skull.

Grad, op. cit. p. 3. [7] Le Prehistorique, 199.
[8] Op. cit. ii. 570.

6. A greyish fresh-water marl with planorbis, paludina cyclas, etc.

M. Favre, in his paper on the beds in the neighbourhood of Geneva, says distinctly that the glacial beds with their erratics, which are greatly developed there, are superimposed on the so-called "alluvion ancienne," i.e. the stratum in which the Pleistocene animals occur; and he adds, "Nous n'avons pu decouvrir dans les environs de Genève aucune preuve de l'existence de deux epoques glaciaires." [9]

At Chambery in Savoy, some lignites have occurred in which although no mammalian remains have been found, plant remains occur, showing them to be on the same horizon as the lignites of Dürnten and Utznach. Their age, says Mortillet, is perfectly established, they are anterior to the great extension of glaciers, whose products, clays, striated pebbles and erratic blocks overlie them. Mortillet continues thus : "Le lignite de Chambéry est tout à fait analogue, comme constitution et aspect physique, à celui des Cantons de Zurich et de St. Gall. Le gisement est semblable et tous les deux sont préglaciaires." [1]

M. Mortillet discusses another instance of which much has been made, and which is a good example of the slight basis upon which interglacial theories have been built. This is an alleged intercalation of stratified "ancienne alluvion" between two glacial beds, in the gorge of the Drance, near Thonon (Haute Savoie). M. Mortillet says the Drance cuts through some almost vertical calcareous beds ; on the upturned edges of these beds there rest layers of alluvion, over which lie glacial beds. In some places in the gorge the calcareous beds have been denuded of their covering, and form a cornice ; on to this cornice some of the glacial débris has slid down and made a kind of talus, looking at first sight as if it were overlaid by the alluvion. The fallacy of this reading was tested by M. Mortillet, by making excavations on the spot,[2] when it turned out that there was no intercalation whatever, but that the alluvion always lay underneath the glacial beds.

If we turn from the northern to the southern flanks of

[9] Bull. Soc Geol. de France ser. iii. vol. iii. pp. 659-660.

[1] Le Préhistorique, 216-217.   [2] Id. 312.

the Alps we shall find precisely the same evidence. At Carignano, at Lanzo near Stura, at Gifflenga in the valley of the Cervo, at Boca, at Maggiora, and at Leffe (Gandino), there are certain lignites which are distinctly correlated, by their mammalian remains and other contents, with the lignites of Utznach, Wetzikon, etc. Now it is a remarkable fact, as Gastaldi has shown, that these Italian beds rest directly upon Pliocene deposits, and in no case, so far as is known, do they have any glacial beds below them.[3]

It seems to me that wherever we can test the question on the flanks of the Alps, we have the same consistent evidence, and that those who have argued from the facts there found that there were interglacial mild climates, have done so from very imperfect data, and by ignoring the substantial case against them.

If we turn to North Germany, we have an area in which the difficulties of correctly reading the facts are very great. In England, where we have magnificent sections of the drifts exposed on so many of our coasts, we shall see how various and contradictory has been the reading of the evidence ; but in Germany the difficulty is greatly increased from the absence of such natural sections. This has been frankly admitted by Mr. James Geikie. We have a further difficulty in that there we have, for the most part, no continuous true till or boulder clay (that unmistakable ice-spoor), and have to fall back upon beds of sand containing boulders, which are so easy to misread, since they may be as easily the products of diluvial as of glacial action. It is to be remarked also that the shells in the German Pleistocene beds are not only much broken and abraded, but are sometimes filled with a material entirely differing from the matrix in which they are embedded, showing, as Geikie says that they are merely erratics, like the glaciated stones among which they occur.[4] The same may be said of a considerable proportion of the bones said to have been found, actually in the glacial beds themselves. These débris, being derivative, afford no evidence of interglacial climate.

Another feature of the German glacial beds, which makes

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[3] See Bull. Soc. Geol. de France, vii. 2nd ser. 544; Atti delle Reale Academie delle Scienze de Torino, viii.

[4] Prehistoric Europe, 278.

them very uncertain indices of climatic succession, is the mix-
ture in so many of them of marine and fresh-water shells, with
remains of land animals. Geikie has remarked upon the diffi-
culties involved in this assemblage of incongruous remains.
They seem to me to show how altered (remanié) and how
disturbed these beds are, and how largely their contents are
derivative, and therefore of very little value as tests. Mr.
J. Geikie admits " that the commingling of discordant species,
and the fact that the specimens of some of the marine molluscs
are much rolled and worn, have led some to doubt whether
they really occupy their original bedding place."[5] The same
conclusion seems to follow from the mingling of northern and
southern stones which must have come from opposite directions
in the drift deposits of Saxony.

It is not easy to directly traverse some of the statements of
German geologists on the subject of interglacial beds,
except on these *à priori* grounds, because their observations
have not been gone over by others, but this is not
universally the case. Thus, Credner, Geinitz and Wanschaffe
have published a paper in the "Neu Jahrbucher" for 1889,[6]
on the age of certain turf deposits of Lauenburg. These
were asserted, by Keilback in 1884, to be interstratified
among glacial deposits. The result of the examination of the
place by the three highly competent geologists is to show that
the bed of turf is not intercalated, as was supposed, but lies *on*
the glacial beds. Movements of the soil and slippings have
misled Keilback, for where the section can be examined intact
it shows a layer of marine sand containing a *cardium*, which
previous observers have treated as of diluvian age, but which
really belongs to the Miocene period ; above which rests a calca-
reous sand which is truly diluvian, and then the glacial marl.
Above this rests the turf, containing lines of sand, above which
there is no glacial bed whatever, and the turf with its sandy
streaks everywhere overlies the glacial beds. The plants it
contains are all still living in Lauenburg, and the bed is really
post-glacial.

This is a most important conclusion, for this particular bed
had been made the foundation of a great deal of reasoning,

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[5] Prehistoric Europe, 281.   [6] Vol. ii. p. 194.

notably by the Commission appointed by the National Geo-
logical Institute of Prussia.

Let us now turn to another area and another explorer.

In the valley of the Inn above Innspruck, at a place called
Hötting, there is a breccia composed of stones from the
upper Triassic dolomite, of pieces of red sandstone and of
crystallized rock. The age of this breccia has been much dis-
cussed, and it was a general surprise to those who knew the
country to find Penck affirming that it was overlaid as well
as underlaid by morainic matter, and was therefore of inter-
glacial age. Ettingshausen, who examined some plant remains
from this horizon, also declared it to be of the same interglacial
age. On the other hand, Unger and Stur, who have examined
the plant remains, have found among them leaves of fan palms
and other evidences of a warm climate, proving the beds to be
of Tertiary age, and therefore outside the possibilities of being
interglacial. I feel convinced that in other cases where the
drift has been supposed to underlie the pleistocene land sur-
face, that tertiary conglomerates more or less disintegrated
have been mistaken for it.

While the theory of a plurality of glacial periods has found
several advocates in Germany, the French geologists are
virtually unanimous on the other side. Falsan, Favre, Desor
and Saporta, are agreed as to the unity of the glacial period,
always qualified with certain oscillatory movements of the local
glaciers. Falsan is especially strong in his opinion; *inter
alia* he says, " Durant nos études avec M. E. Chantre sur le
terrain erratique du basin du Rhône dans sa partie moyenne,
nous n'avons pas trouvé de traces de deux ou de plusieurs
périodes glaciaires. . . . Nous avons été amené ainsi à n'ac-
cepter qu'une seule période glaciaire d'une durée immense; "
and, after granting certain oscillations, he continues :
" Nous repoussons, comme peu vraisemblable, la theorie des
récurrences de périodes glaciaires distinctes." [7]

Let us now turn to an area where the question can be
examined under the most satisfactory conditions, and where
recent explorations have greatly modified the older views.

Mr. H. B. Woodward, in his paper on the Glacial Drifts of

La Période Glaciaire, 211, 351.

Norfolk, refers to the so-called middle glacial sands and gravels on the East Anglian coast containing fragments of more than a hundred species, identified by Messrs. Wood and Harmer as Crag shells. The presence of these shells pointing to warmer conditions, and the conclusion of the observers just named, that they were not derived but contemporaneous, first started, according to Woodward, the notion of inter-glacial warm periods; for the presence of Coralline Crag forms suggested that some connection with the Mediterranean area had been opened by submergence of the land at this period. This view was also maintained by Mr. Clement Reid, in his memoir on the Geology of the Country round Cromer. Woodward traverses the conclusion. He says the aspect of the shells alone makes one sceptical, and it is admitted that they did not live on the spots where they have been accumulated. " These beds pass southwards into gravels which underlie the chalky boulder clay ; and he further says of these gravels that they do not indicate deeper water conditions, nor have we any southerly fossiliferous beds of this age, which we might expect had the fauna migrated in interglacial times from the Mediterranean area, and he suggests that the middle glacial shells may have been largely derived from old Crag accumulations, now entirely destroyed or buried beneath the waters of the North Sea."

Mr. Clement Reid, who, as I said, formerly held a different view, writes to me : " The fauna of the middle glacial sands of Norfolk, I now have no doubt, is entirely derivative. Formerly, I accepted Searles Wood's view as to the con-temporaneous age of the mollusca, because of the apparent force of his argument from the perfect preservation of thin and delicate shells. The argument I now see has no value, for shaken about *under water* thin shells are less injured than thick, massive ones. The extraordinary mixture of species belonging to different sea-bottoms, zones of depth, and climates is also against the acceptance of the fauna as a homogeneous one."

In an elaborate paper on the Drifts of Flamborough Head, Mr. Lamplugh, who is a strong believer in glacial phenomena on a great scale, has recently discussed the evidence of the East Yorkshire drift beds, with great minuteness and skill.

In his paper he points out the very favourable position of this area for solving the riddles of the glacial age. " If the glacial history of an area thus laid open cannot be deciphered," he says, " there must indeed be slight hope for less favoured localities." After referring to Mr. Searles V. Wood's interpretation of the sections, he continues : " It was with a learner's faith in this view, and with a similar faith in the recurrence of 'interglacial' periods, that I commenced the study of the Flamborough sections, over twelve years ago. But with the gradual accumulation of opposing evidence, my confidence was weakened, until at length I have been driven to contrary conclusions on both points." [8] Speaking of a section at Speeton he says : " Had there been, as has been supposed, a long mild interval between the formation of the different Boulder clays, the older deposits would scarcely have withstood erosion, and remained on the crest of this steep escarpment." [9] In regard to the beds at Sewerley and Speeton containing Pleistocene remains, he shows that they underlie the Basement Clay, the lowest of all the glacial beds, and that they are therefore preglacial, and not interglacial, as has been argued.[1] Again he argues : " If there had been in Yorkshire an interglacial period of so long duration as to allow the sea to cut back a line of hard chalk cliffs, with a broad tidal platform at their base, its effect in other areas must have been equally marked. Yet I am aware of no evidence for such an interval among the drift deposits of the East of England." [2] Again he says : " From the foregoing account it will be gathered that, in my opinion, there is no good evidence in East Yorkshire for a mild interglacial period, but that, on the other hand, the formation of the drifts, from the Basement Clay upwards, was directly dependent upon the presence of land ice in the bed of the North Sea, and went on uninterruptedly, though with wide fluctuations of the ice margin, to the closing stages of the period. . . . There is no clear evidence on Flamborough Head for a mild interglacial period, but only for extensive fluctuations of the margin of the ice." [3]

From these extracts it will be seen on what a slender and vanishing basis the theory of a mild period intervening

[8] Quart. Journ. Geol. Soc. xlvii. 385. [9] Id. 405.
[1] Id. 411-12. [2] Id. 413. [3] Id. 427-429.

in the glacial history of the Yorkshire beds rests, and that, as judged by the fauna, the beds instead of supporting such a view are at one with those of East Anglia.

If we turn to the deposits in the West of England, we shall find the testimony of patient observers the same. Thus, in a paper on the drift beds of the N.W. of England and Wales, Mr. T. Mellard Read says : "The middle sands and gravels are supposed by some to represent 'interglacial,' and by others 'milder' conditions, in consequence of the stones being all rounded and unglaciated. If this be the case, what do the sands and gravels of St. Bees represent ? They are even a more distinct deposit, and overlie a lower clay with an eroded surface ; but here the conditions are reversed, for the sands and gravels contain in places more and larger blocks than 'the Lower Boulder clay' of Blackpool, and they are full of contortions and evidences of violent action of some sort. "[4] Again, referring to the low level boulder clay and sands, he notices that forty-four species of shells were found in a typical section at Bootle Lane, which were increased by Mr. Stone's finds at Newton to fifty-six. Sixteen species of Foraminifera, besides Ostracoda and other minute reliquiæ of the sea, had also occurred in these beds. " If, as some maintain," he continues, " two glacial and one interglacial periods are represented in these beds, there ought, à *priori*, to exist some decided distinction in the molluscous fauna. I utterly failed to detect any, nor is it maintained that any subsequent observations have succeeded, though the observers have been many and zealous, and only anxious to make the discovery."[5] Again, in reference to the stones being more rounded in the sands and gravels, he reiterates his opinion " that this can hardly be evidence of an interglacial climate ; for we find in many cases that the lower and harder beds of the clay often, nay, generally, contain more rolled stones than the upper or middle. Such is the case at Dawpool, and it was so in the Atlantic Docks, Liverpool. At Blackpool, on the contrary, according to Binney, the lower and harder beds contain the greatest number of striated stones. It is therefore clear that these distinctions cannot indicate climatic differences."[6]

[4] Quart. Journ. Geol. Soc. xlvii. 111.   [5] Id. 124.   [6] Id. 125.

Mr. Mellard Read speaks thus in another paper : " There is, according to shell evidence, nothing to give an inter- glacial character to these gravels as compared with either the so-called Upper or Lower Boulder clays of the north- west of England. My opinion has long been against this tripartite classification, and I have pointed out more than once that it rests upon no intelligible basis. The examina- tion of the Irish Drift still further confirms me in the opinion that the marine Boulder clays of the north-west of England and Ireland are but phases of one long sequence of events, uninterrupted by changes of climate."

If we turn to Scotland, the evidence proves to be equally doubtful and fragile. Thus, in regard to the Pleistocene mammals, the view is now generally accepted that, in every case where they have been found in a contempo- rary bed, that bed underlies the till, and is therefore pre- glacial. As in other places, so here, teeth and bones of mammals have occurred in the clay itself; but in all such cases they occur sporadically and as boulders. As Mr. James Geikie says, " They almost invariably afford marks of having been subjected to the same action as the stones and boulders by which they are surrounded ; that is to say, they are rubbed, ground, striated and smoothed." [7]

This assuredly points to these bones having been origin- ally preglacial and not interglacial.

If we turn from the evidence of the mammals to that of the plant beds in Scotland, of which so much has been made by Mr. Geikie, I cannot quote a more weighty authority than Mr. Clement Reid, who has devoted so much time to their study, and who commenced that study with predilections in favour of Mr. Geikie's views. He writes to me as follows :—

" As regards the Scottish interglacial plant beds, the character of the flora from the well-known localities of Cowden Glen, Redhall, and Hailes, seems fatal to the acceptance of an interglacial age for the deposits. Each of these localities yields several plants which every botanist concedes only to occur in this country as weeds of culti- vation. From Cowden Glen were obtained seeds of the

[7] Great Ice Age, 129.

opium poppy, a most unlikely plant to be a true native of
Britain. At Hailes, much charcoal occurs. There are also
thin seams of peat intercalated in the Boulder clay of
different parts of Scotland, but these yield a flora only
remarkable for its extreme poverty and for including only
plants of wide range. If, as seems not improbable, these
are really included in the glacial deposits, they are not suffi-
cient to prove a mild climate."

Mr. Somervail writes on this subject in very emphatic
terms. Thus he says: "That these so-called interglacial
periods have no substantial existence, we will now attempt
to show. In whatever part of the country we examine the
till or boulder clay, we discover it to be most variable in
all its characters. Those intercalated beds on which the
interglacial periods are founded are sometimes entirely
absent, even in localities where it reaches a very considerable
thickness. There is, I think, overwhelming evidence that
to found interglacial periods, each of several thousand years
of duration, on these inconstant, variable, irregular, and
patchy deposits, is strikingly and palpably absurd. It is a
most remarkable fact that many of the so-called interglacial
beds have yielded abundance of marine shells which show a
much colder set of conditions than those actually inhabiting
our present seas. Thus, in spite of the alleged warmth
and high temperature, the whole evidence furnished by the
mollusca obtained from the intercalated deposits is decidedly
in favour of conditions approaching to an Arctic type.
Had the so-called interglacial beds contained such genera
as *Terebra, Conus, Oliva* or *Voluta*, the evidence for warmer
conditions would have been worth considering; but when
we find such genera and species as *Astarte borealis, Tellina
calcarea, Leda pernula*, and many others which now are only
to be found in the Arctic seas, occurring in the interglacial
deposits, then those warm and sub-tropical periods dis-
appear 'like the baseless fabric of a vision.'"

The point here made by Mr. Somervail seems to me an
excellent one. If there were interglacial mild climates, they
must have affected the seas no less than the land, and we
ought to find among the mollusca—not from different and
separated areas, but from beds superimposed on each other

and marking different types of sea—plain testimony of changes of climate, more easy to discriminate than the ragged and broken fragments of old land-surfaces.

Having shown how the European evidence dissolves away when examined, let us conclude by turning to America.

The American evidence is really very fragile; as Mr. G. K. Gilbert, a distinguished American geologist, says, "If the hypothesis (Dr. Croll's theory) is true, the cold of the Glacial epoch must have been many times interrupted by intervals of exceptional warmth. . . . In America, where there is now great activity in the investigation of glacial phenomena, the evidence of *a single* interglacial period is cumulative and overwhelming, while there is no indication whatever of more than one." [8]

Even this one, however, is very doubtful. Dr. Wright speaks of President Chamberlin as having a wider knowledge than that of anyone else of the facts. Chamberlin says, "Certain subaqueous deposits so closely resemble true till that they have been mistaken for it, *and there is perhaps no case of superposition of beds supposed to represent two glacial periods that is not still open to these doubts.*" [9]

If we turn to the organic evidence supposed to prove mild interglacial temperature in America, it consists almost entirely of remains of trees found in well-borings or in other sections in the midst of the glacial beds. In the very great proportion of cases, however, this evidence is merely that of the occurrence of detached logs of timber, which, in my view, is no evidence at all. Dr. Wright says of them, "In case of many of the fragments and logs of wood found in the glacial deposit, we are not compelled to suppose an interglacial origin. Wood will stand transportation in the ground moraine almost as well as boulders, and it is by no means certain that much of the timber found in the till may not have belonged to the original forests, which covered the country in front of the first sheet of advancing ice. These logs may have been picked up like the boulders, and trans-

[8] Nature, xxvii. 262.
[9] Geology of Wisconsin, i. 272.

ferred to the south a long time after their original de-position." [1]   This is precisely my view.

The difficulty of arriving at a really definite conclusion may be gathered from the following paragraph of Mr. James Geikie's. He says very frankly, " It is curious to find that the geologists of Illinois, Indiana, and Ohio have different views as to the position of the old forest beds. . . . The geologists of Indiana apparently recognize only one forest bed, and it underlies the drift. On the other hand, the geologists of Ohio assert that their forest bed is intercalated between glacial deposits, for it rests upon tough boulder clay which is full of striated stones, while over it come accumulations of clay, sand, gravel, and erratics. Again, in Minnesota, the forest bed reclines upon a mass of blue boulder clay with striated stones, and is overlaid by an upper accumulation of precisely the same kind of material." [2] This is assuredly a very embarrassing dilemma for those who have to try and make out an affirmative case for interglacial mild periods, and I cannot at all accept the ingenious special pleading of Mr. James Geikie to account for it. But Mr. Geikie over-states the case. Take the evidence from Illinois, for instance. In Perry County, Illinois, Mr. Worthen describes a bed of blue sand with leaves and sticks as occurring below the main mass of drift. This bed, he continues, usually lies at the bottom of the drift deposits, but at one point in Jackson County it was underlaid by *a bed of sand two or three feet in thickness.* It is assuredly a brave thing to postulate that a bed of sand is in itself any proof at all of a glacial stratum. In Woodford County a similar accumulation of peaty matter was found, at a depth of about sixty-five feet, *below fifty feet of hard-pan.* [3]

In Ohio the evidence, although contradictory, does not by any means seem to bear out the conclusion of Mr. Geikie. In an interesting paper by Mr. Charles Whittlesea, entitled " Notes upon the Drift and Alluvium of Ohio and the West," he tells us that in 1844 he examined fifty-nine wells in different parts of Hamilton County, in that State, of which only six or ten per cent. showed traces of dirt beds, leaves, timber or

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[1] The Ice Age in America, 477-478.   [2] Great Ice Age, 455.
[3] Great Ice Age, 451.

silt.   He mentions several instances of the occurrence of logs
and pieces of wood embedded in the hard-pan clay, but in
all these cases they are clearly derivative and as much true
boulders as the stones that lie alongside of them, and have
come from some antecedent beds.

In several cases where there was an accumulation of leaves,
etc., showing therefore something like an old land-surface,
it was generally at a depth of thirty feet, and the beds are
said to have had strata of blue and other clays over them
but not under them.   I will quote an instance.   Three miles
north of New Burlington a bed of leaves was passed at
thirty feet from the surface.   Another, at forty feet, had leaves
and logs under the blue clay.   Another gave the following
section :—

		Feet.
1.	Surface loam	1.
2.	Yellow clay	3.
3.	Sand	3.
4.	Blue clay	14.
5.	Leaves and sticks.	

At a place called Mercer, at the source of the Little Miami
River, timber was found forty feet deep in the blue clay, and
a dirt bed at fifty feet (i.e. the dirt bed underlay the blue
clay).   " In 1838 Col. John Madina of Chillecothe showed me
a piece of wood taken, as he said, from a well on the hills
east of the Scioto, about 300 feet above the lake, at the depth
of thirty feet.   It was a foot long, one and a half inches
through, hard, brittle and resinous, of a dark colour, *and was
overlaid by clay."*

This evidence if not conclusive points to the so-called
dirt beds of Ohio having been really pre-glacial.   Professor
Wright, whose moderation and whose clear-headedness are so
conspicuous, says : " A thorough study of the condition and
distribution of the buried forests bears strongly, as I cannot
but think, against the complete separation of glacial epochs in
North America. . . . It is a significant circumstance that the
buried vegetable deposits under consideration do not mark a
warm climate, but a climate much colder than the present ;
such a vegetation, in fact, as would naturally flourish near the
ice margin.   The buried forests of Southern Ohio have a
striking resemblance to those we described in Glacier Bay,

Alaska. Peat and hardy coniferous trees are predominant." [4]
How is this consistent with an interglacial mild climate ?

Dr. Wright not only writes with force, but he has given us
some admirable illustrations of glacial phenomena, not sophis-
ticated by human pencils, but taken by that most neutral
observer the sun ; and *inter alia* he has given three sections
from Ohio to illustrate this part of his subject. In one, a piece
of wood is seen projecting from the centre of a mass of till
about fifty feet in height, exposed at Oxford, Ohio, and he adds
the note : "This has evidently been transported in the till
like a boulder." [5] In another case is a section of a perpendicular
bank of till ninety-five feet in height, near Germantown, Ohio.
This is a splendid and most typical section ; *underneath* the till
there is a deposit of peat fourteen feet thick. Both the
stratum of peat and the clayey till above contain many frag-
ments of coniferous wood, some of which can be identified as
red cedar (*Juniperus virgineanus*). It seems to me that the
pieces of cedar wood scattered through the till are all of them
derivative, and that they came from the disintegration of the
peat bed. In a third section, at Parrstown, in Butler County,
Ohio, there is an exposure of till sixty-five feet high, con-
taining fresh cedar logs *near the bottom* and fragments of wood
in all conceivable positions throughout the lower half of the
beds ; a line of stratified deposit runs through the till and
the fragments of wood occur above as well as below this line.
The fragments, we are told, found both above this stratum and
below it are identical in kind and in the same state of preser-
vation. [6] Would it be possible to find a more signal instance
of the pre-glacial character of the bed and of its having
been disintegrated by the same agency that distributed the
till ? Professor Wright gives a number of other cases, but
in nearly every one they are instances of wood having been
found underlying the till.

I have now completed my survey of the witness of past
geological time to the question of intermittent climates, and
it seems to me the case for them utterly breaks down. We
may traverse the whole world, and whether we use the fauna
and flora as a test and measure of climate, or turn to the more

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[4] The Ice Age in North America, 482.   [5] Id. fig. 115.
[6] Id. 489, 490.

solid and indestructible inorganic evidence, we shall fail to find a support for the far-reaching hypothesis that the earth has passed through a cycle of glacial and temperate periods alternating with each other. It follows, therefore, that the evidence of geology and palæontology is at one with that of astronomy and meteorology, and that both are equally opposed to the notion that glacial epochs have been recurrent events in the world's history.

CHAPTER XII.

THE GEOLOGICAL WITNESS AGAINST ALTERNATE GLACIATION IN EACH HEMISPHERE, AGAINST A GLACIAL PERIOD IN THE SOUTHERN HEMISPHERE, THE GLACIATION OF THE TROPICS AND POLAR ICE-CAPS.

" ' O meu padre,' was the exclamation of a Brazilian as he introduced Agassiz to his parish priest, ' O meu padre, here is a wonderful worker of miracles. He can fill these valleys with ice, he covers the mountains with snow in the hottest days, he brings the stones from the top of the serra to the bottom, he finds animals in the bowels of the earth, and brings out their bones.' ' Ah,' replied the padre, ' he is a wonderful saint truly. He is such an one as I need for my chapel."—Trans. Ed. Geol. Soc. 1882-3, 256.

No evidence of alternate glaciation in each hemisphere—Southern development of glaciers contemporary with northern : Agassiz, Belt and Wallace—Earth compared with Mars—Cause of greater cold in the Antarctic regions—No evidence of an ice age in New Zealand, Australia, and South Africa—Distribution of plants and animals against it—Views of Haast, Hector, Hochstetter, Hutton, Von Lendenfeld, and McCoy—The case against tropical glaciation— Impossibility of the emigration of the tropical animals and plants —Views of Wallace, Orton, Ricketts, Hartt, Conrad, Crevaux— Occurrence of boulders in the tropics explained as the result of torrents : Schomberg, De La Beche, W. T. Blandford, Wynne— The case against polar ice-caps—A priori arguments : Reclus, Nordenskiold, Lyell, Wallace, Ricketts, Hooker, Wilkes—Croll's reply untenable—Evidence from the polar regions : Reclus, Lyell, Wallace, Thompson, McClure, Fielden. Nordenskiold—Traces of glaciation only found in one half of the circumpolar area : Nordenskiold, Dall, Wright, Fielden, Tristram, Campbell—Evidence that the ice in the Arctic regions flowed northwards and not southwards : Böhtlink, J. Geikie, G. Dawson, Haughton, Bessels, Bell —Existence of unweathered and prominent rock pillars, etc., in the Bear Islands—General conclusion against polar ice-caps : J. Geikie, Fielden, Chamberlin, and Judd.

IN the previous chapter I have tried to sift the evidence furnished by geology, in regard to glacial periods having been recurrent in former times, and have shown upon what a very small basis the theory has been built. Let us now advance another step.

It is plain that, if the views of Croll and others are main-

tainable, the two hemispheres of the earth must have been alternately subject to glacial and to temperate conditions, the one being dominated by the sun and enjoying perennial spring, while the other was dominated by frost and under glacial conditions, and we ought to find some geological evidence of the fact. I know of none.

The opinion of the local geologists in the southern hemisphere is almost unanimous that while it is probable its great glaciers have a continuous history going back to Pliocene times, which may have been the case in the northern hemisphere also, they were the immediate precursors of the present ones,[1] and that neither the remains of the fauna, nor the loose deposits, furnish traces of an intervening stage marked by another climate, or justify any other conclusion than that the development of the southern glaciers was contemporaneous with the development of the northern glaciers, and neither preceded nor was subsequent to it.

On this point Agassiz is very emphatic—

" I have not noticed anything," he says, " to confirm the idea that the glaciers of the northern hemisphere have alternated with those of the southern hemisphere in their greatest extension, as is assumed by those who connect with the precession of the equinoxes the difference of temperature required for the change. The abrasions of the rocks seemed to me neither more nor less fresh in one hemisphere than in the other; nor do the veins of molten rocks rising above the surrounding disintegrating rocks stand out in a more or less bold relief in either case. However astronomical causes may have been connected with the climatic conditions of the world, I see no reason for believing, from any facts I have observed, that alternations of temperature in the northern and southern hemispheres have ever been the primary and efficient causes of glacial phenomena."[2]

Mr. Belt says: " I think it highly probable that the evidence that is accumulating will force geologists to the conclusion that the ice of the glacial period was not only more extensive than has been generally supposed, but that it existed at the same

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[1] See Haast, Geology of Canterbury, 372, etc.; Dobson, the Date of the Glacial Period in New Zealand, Trans. N. Z. Institute, vii. 440, etc.

[2] Nature, vol. vi. 272.

time on the northern and southern hemispheres, leaving at least on the American Continent, only the lower lands of the tropics free from the icy covering."[3]

Mr. Wallace says that " the traces of ancient glaciers in New Zealand point to a period so recent, that it must almost certainly have been contemporaneous with the glacial epoch of the northern hemisphere."

Nor is this alternating theory supported by an appeal to what we know of other bodies than the earth, which have an orbit of marked eccentricity, and are therefore in a condition like the earth must have been during the supposed glacial period. Fortunately, as Professor Bonney has suggested, it is easy to make such an appeal. The planet Mars, so far as we can gather, is constituted like the earth. We can trace out its land areas and its seas, and also see its atmospheric effects. Among other things which it contains are two white polar caps. These were observed by Herschell to shrink and enlarge with the seasons, and there can be little or no doubt that they represent the ice at either pole of Mars. Now, in addition to its physical resemblance to the earth, Mars fulfils the conditions demanded by Dr. Croll, namely, its orbit is very eccentric, and the inclination of its axis shows great obliquity, yet nothing is more certain than that the results suggested by Dr. Croll are not forthcoming. We have not one pole with a gigantic ice-cap marking its ice age, and the other denuded of ice, marking its inter-glacial period, but each pole acquires in winter a great cap of snow, which does not accumulate, but is almost, if not quite, melted away in the succeeding summer. There is thus an alternation of extreme conditions at each pole with each season, but no trace of an ice age. The example is a crucial one, for under no conditions could the earth approach the favourable climatic circumstances of Mars for fulfilling Dr. Croll's demands. Its southern summer is passed in perihelion with 296 Martial days, while its winter is only 76 days long. In the other hemisphere these numbers are reversed.[4]

This being the evidence on one side, there is literally none known to me on the other, except the fact that the southern hemisphere, whose winter is now being passed in perihelion, is

[3] Nature, vol. vi. 264-5. [4] Pilar, Ein Beitrag, etc., 68.

undoubtedly colder than the northern hemisphere, and the Ant-
arctic climate is more severe than the Arctic ; but this has been
explained by the greatest authority on such a question, namely
Dove, in a most satisfactory way. He has shown that it is due
not to any astronomical cause, but to the much greater quantity
of land in the northern hemisphere than in the southern, which
absorbs more of the sun's rays, and which actually causes the
earth's equator of heat to be situated some degrees north of
its equator of form. It is the result of the distribution of land
and water over the world, and this, so far as we can judge,
was virtually the same during the prevalence of so-called
glacial conditions in the northern hemisphere. Dr. Croll
himself admits that there has been virtually no change in that
distribution since glacial times.

We may take it, therefore, that in a problem in which there
is necessarily no conclusive evidence either way, all the evi-
dence there is is on one side and against the contention of those
geologists who appeal most persistently to astronomical causes.
Not only is there no evidence of alternate glaciation, but, as
the term glacial period is generally used, I don't know of any
evidence that there ever was an ice age in the southern hemi-
sphere at all.

The first argument I would use is not a geological one.
In the southern hemisphere we have more than one
district, notably Australia, New Zealand, and the Antarctic
islands, each characterized by very peculiar forms of life, both
animal and vegetable, whose existence is inconsistent with a
glacial climate. What became of them, and where did they
migrate to when the southern ice-sheet, which Agassiz, Dr.
Croll, and others appeal to, was overwhelming those countries ?
The same argument applies to the inhabitants of the sea in
these latitudes. Again, if such a change of climate had
occurred, we should assuredly have found traces of it in the
fossils, both the marine and land fossils, such evidence as we
have abundantly both in Europe and North America. But
all such evidence is absolutely wanting.

It is clear, again, that if the cause was a similar and recurrent
one in either hemisphere, as urged by Croll and those who
maintain Croll's position, we ought to be able to support it
by similar evidence, not only from one hemisphere but from the

other. We cannot doubt that the very same cause, operating on the southern hemisphere, must have had the same or similar results to that operating on the northern one. We ought to find in the southern hemisphere, therefore, a state of things corresponding to that which is a marked feature of the northern hemisphere, namely, a vast development of glacial phenomena, such as polished and rounded rock-surfaces, striæ, erratics, and drift, not limited to a few valleys once occupied by large glaciers, but spread over an enormous area, and needing no microscope to discover them. Where are we to look for anything like this in the southern hemisphere, and especially in that part of the southern hemisphere which forms the *antipodes* to the North Atlantic district? We undoubtedly have at this moment with no excessive eccentricity a remarkable development of cold at the South Pole. With greatly increased eccentricity, if Dr. Croll's arguments are good, this ought to have been greatly increased, and especially ought it to have been so in the southern hemisphere, where there is so much water, and, consequently, so much evaporation. But where are we to look for it? In South America we no doubt have something resembling what we can find in boreal America: boulders, erratics, and scratched rocks, which have been described by Darwin, Agassiz, and others. But that these evidence a development of large glaciers and not a glacial period, is shown by the fact that the very elevated raised beaches in South America contain the same molluscs as those still living in the adjoining seas. But granting that there is some evidence in South America, what of New Zealand (our *antipodes*), Australia, and South Africa? It is quite true there is ample evidence that, both in New Zealand and Australia, there were formerly glaciers on a greater scale than any now existing. On the other hand, the testimony of most careful and competent observers is virtually unanimous as to there being no such traces of a glacial age there as we find in the northern hemisphere. As this is a matter of some moment, I must be allowed to quote some authority for my statement. In regard to New Zealand, Haast, Hector, and Hochstetter are agreed that the evidence is clearly in favour of the former great extension of glaciers, and against any ice-cap or so-called glacial period.

Haast, who once advocated the opposite view, abandoned it in 1864. Writing in 1865, he says his investigations led him to the conclusion that the enormous glaciation of New Zealand in Pleistocene times was due to the existence of plateau-like mountain ranges, lying above the level of perpetual snow. He was confirmed in the view that the glaciation was that of glaciers by finding in several localities the bones of *dinornis* and *palapteryx,* either among the Pleistocene terminal and lateral moraines themselves, or in the lacustrine deposits found immediately after the great glacier had retreated. The position of the bones convinced him at once that they occurred in the localities where they had originally been deposited, and not in reassorted strata. Bones of the same species occur in the New Zealand Alps, wherever favourable conditions exist for their preservation, partly covered by vegetable soil, or even quite free among the grass and shingle as if they had only recently died . . . . "It is not unreasonable," he says, "to deduce from such an important fact that, from the Pleistocene epoch to the present day, the flora of our sub-alpine and alpine ranges has not undergone any material change, and that the plants which in the recent era have doubtless offered ample food to these wingless giants, are the same as those in which they existed in former times." He concludes that the existence of the Moa bones found among the Pleistocene terminal and lateral moraines induces us, more than any other reason, to adopt the conclusion that nearly the same meteorological conditions existed then as now ; offering at the same time a further reason for accepting the theory of large plateau-like ranges rising above the line of perpetual snow, as a principal cause for the greater glaciation of New Zealand during the Pleistocene period.[5]

I ought to add that the very peculiar semi-tropical fauna and flora of New Zealand and Australia, pointing to isolation from early geological times, points also to the fact that there have been no recurrent glacial epochs in the southern hemisphere.

Mr. Travers is very decided in his views on the subject. He shows that the major part of the New Zealand flora has a semi-tropical character, inconsistent with the probability of

[5] Quart. Journ. Geol. Soc., xxi. 135-137.

its co-existence with severe glacial conditions, many of the most hardy plants being unable to withstand the rigour even of an English winter. " The wingless birds, both existing and Pleistocene, which cannot have migrated, are also incompatible with such conditions. Again, the careful comparisons which have been instituted between the existing mollusca of our seas and fossil species, extending in age from recent up to Upper Eocene times, has failed to indicate the occurrence of any such glaciation. The number of Pleistocene fossils obtained has been very considerable, and a large proportion of them was obtained from the South Island, in localities in which evidences of such glaciation would have been found in abundance if the glaciation itself had ever existed." [6] No morainic accumulations are to be found on the Canterbury plains, except at very short distances below the mouths of the greater valleys. No boulder clays are to be found of the kind which would result from the action of an ice-sheet. Nowhere on Banks' Peninsula are to be found the smallest traces of glaciation.[7] Elsewhere he says : " For my own part I have never seen, at least in those portions of the South Island mountains which I have personally visited, the slightest evidence which would support such a statement as that it had once been subject to glaciation like that of Greenland, or which would have led me to the belief that even during the greatest elevation of the land of which any indications remain, it presented features of glaciation, differing (except in such a degree as would naturally follow in this latitude), from those which it now presents, where glaciers of the first order still exist.[8] Such a glaciation as above suggested must necessarily have obliterated all but the scantiest fragment of the fauna and flora of the country, leaving at most but a few Alpine forms struggling for existence amidst the inhospitable conditions by which they were surrounded ; whilst, on the other hand, the study of existing forms of life, and of those which have certainly become extinct within pleistocene times, has led all who have engaged in it to a conclusion entirely at variance with any such assumption." [9]

Hector, writing in 1870 of New Zealand, says, " The theory

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[6] Trans. New Zealand Inst., vii. 419-433.
[7] Id. 434.   [8] Id. vi. 303.   [9] Id. 304.

of an ice-cap during the glacial period . . . . is quite irre-
concilable with observed facts, and the former extension of
the glaciers is sufficiently accounted for by the gradual re-
duction of the surface area exposed above the perpetual snow-
line : firstly, by its erosion into valleys, ridges, and peaks ;
and secondly, by its gradual subsidence, a subsidence which
has operated for the most part continuously, though inter-
rupted by irregular and local elevations." [1]   Similar views
were urged by Hochstetter in his work on New Zealand,
1867, p. 505, where he says, "In explaining the cause
of the Pleistocene glaciation of the South Island of New
Zealand, we must not resort to the hypothesis of a general ice
period caused by cosmical influences . . . but find the most
reasonable explanation in physical causes now in existence, if
we only suppose the Southern Alps during the pleistocene
period formed higher and more plateau-like ranges, while the
climatic influences as well as the action of water and ice at
that period were the same as at present."

In a paper by Captain Hutton (who studied the geology of
New Zealand so well), which was printed in the 12th volume of
the "Geological Magazine," he absolutely denies the existence
of evidence of the former existence there of a glacial period ;
he points out that at Wanganui, in Cook's Straits in New
Zealand, there is the most extensive Pleistocene shell-bearing
bed on the island, which has been well worked. From this
bed ninety-one species of shells are known, of which eighty-one
are still living in the seas of New Zealand.   "There is, there-
fore," he says, "no evidence of reduction of temperature in
the early part of the Pleistocene period."

Below this Pleistocene bed is a blue clay in which
ninety-eight species of shells have occurred. Of these seventy-
seven still inhabit the New Zealand seas, and Captain Hutton
consequently attributes them to the newer Pliocene period.
He says : "I have travelled over and mapped the whole
of the province of Otago and I have met with no stratified
till, or any marine beds intercalated between glacial or glacier
deposits ; although since the Pleistocene period the land has
been undergoing elevation. On the whole, therefore, the
evidence is decidedly against the idea that a colder climate

[1] Geol. Mag. vii. 95-6.

formerly obtained in New Zealand. . . . As New Zealand is nearly antipodal to Great Britain, any change of climate in one place, caused by a change in the position of the earth's axis of rotation, would also necessitate a similar change in the other place." He concludes: "There is no evidence whatever of a glacial epoch having occurred in New Zealand, although, if it had occurred, there is every reason to expect that it would have left sufficiently clear traces behind it." Professor M'Coy has also come to the conclusion that there was no glacial epoch in Victoria. He says, " All our evidence in fact goes to show that there was no glacial epoch in Victoria succeeded by a warmer modern one, but there has been a regular and gradual falling of the temperature to the present day." [2]   In support of this conclusion he elsewhere quotes the occurrence of *Areca sapida,* living on Banks' Peninsula, of *Paryphanta Hochstetteri* in Nelson and Picton, of *Ranella leucostoma* and *Cassispyrum* in Martin's Bay, and of *Triton Spengleri, Scalaria Telebori, Cookia sulcata,* and others at Stewart Island as outliers, which have been isolated by a gradually cooling climate.[3]

Mr. R. von Lendenfeld tells us in a paper read before the Geological Society of London in April, 1885, that it was only in the highest ranges of the Australian Alps he could find traces of ice action. No traces of ice action could be found below 5800 feet above the sea. This shows the traces are of old glacier action and not of a glacial period.[4]

Captain Hutton, in a paper on the supposed glacial age in Australia, published in the Transactions of the Linnean Society of New South Wales, discusses the supposed evidence on which it has been propounded, and shows that it is susceptible of a different interpretation, and that it points, as in New Zealand, to former glaciers on a larger scale, but not to an ice age.

He points out that while roches moutonnées and smoothed surfaces of decomposed granite have been found by Dr. Von Lendenfeld high up in the Australian Alps, no moraines have occurred there. Even if these be traces of ice action and

[2] Annals Nat. Hist. 3rd ser. vol. xx. 194.
[3] Proceedings Lin. S., New South Wales, x. 338.
[4] See Geol. Mag., sec. iii. vol. ii. p. 281-2.

not of granitic expoliation, they only evidence old glaciers, and not an ice period.

In regard to certain granitic boulders on the beach at Adelaide, which Dr. Von Lendenfeld considers were transported from the Pole by icebergs, Captain Hutton asks, if so, why are not similar ones found in Tasmania, New Zealand, and the Antarctic islands? How, again, is it that they are all of granite, and do not consist of volcanic rocks, of which all the land south of Australia, hitherto examined, consists? If the boulders came from Tasmania or New Zealand, gneiss, or schist, or sandstone ought to abound, and granite to be rare. He suggests that these boulders may in fact be mere ship's ballast. No doubt old moraines exist at a lower level than modern ones in New Zealand ; but this does not prove a change of climate, and the descent of the glaciers to the sea. It merely shows that the land then stood at a higher level, which is confirmed by many facts, and he concludes that the same was the case in Australia.[5]

Mr. Howitt says : " Nowhere in Gippsland have I been able to detect any appearances which I could in any way refer to a glacial period, analogous to that of the Northern hemisphere. I have nowhere met with grooved or scratched rocks, erratic boulders, moraines or any traces of ice action, and I think that had such existed they would have been met with ere this. Mr. Selwyn has, I believe, already noted this."[6]

Mr. J. E. Tenison Woods says, " There is no satisfactory evidence of any former participation in the great ice-age by the continent of Australia. One or two instances of grooves or striations are recorded; but standing alone in so vast a territory their ice-origin is very doubtful. On the whole, the evidence afforded by the animal remains is decidedly in favour of a warmer climate for Australia than that which now prevails, and this is borne out by the plant remains.[7]

What is true of New Zealand and Australia is true also of Tasmania, where I believe no moraines, etc., have been found. In regard to South Africa, where Mr. Stow professes to have found some glacial gravels, Mr. Griesbach, who examined the district, disputed the possibility of any of the gravels of

[5] Op. cit. 334-341.
[6] Q. J. G. S., xxxv. 35.   [7] Proc. Lin. Soc. of N. S. W., vii. 380.

the Vaal being of glacial origin.[8] I might continue, but this evidence, in which all the most experienced geologists concur, will suffice to show that there is nothing in the southern hemisphere corresponding to the glacial phenomena so conspicuous in the northern one, and this fact is fatal to such theories as Dr. Croll's, which are based upon the recurrence of similar conditions in either hemisphere alternately. The only evidence we have from the southern hemisphere, and of that there is abundance, goes to show that in Pleistocene times local glaciers were developed on a much larger scale than now, and co-existed alongside of a similar vegetation now prevailing, proving anything but a critical change of climate. Let us now pass on.

The advocates of the glacial theory in its advanced form, and notably Agassiz, Croll, and Dana, have necessarily appealed to the existence of a great accumulation of snow, in each hemisphere alternately, forming what they call ice-caps. These ice-caps are more or less a necessity of their theory. The only difference between them is as to their extent, some bringing them down into temperate regions only, while others, like Agassiz and Belt, take them into the tropics. All these writers treat them as having culminated at the poles, and spread equably or more or less equably all round, in all longitudes, forming a terrestrial parallel to the ice-caps of Mars.

Let us first examine the more extreme view involving tropical glaciation.

The first and most conclusive argument against it is what we find prevailing in Mars at this moment, where with conditions far more favourable than ever existed on the earth, we have no trace of tropical glaciation, but the ice caps are limited to the polar areas.

Reverting to our own planet, let us first turn to the biological argument. The areas over which the tropical ice-sheets are supposed to have flowed, are the homes of the most profuse tropical vegetation and tropical animal life that exists anywhere. Where did this withdraw to, during the supposed glacial dominance? The orchids and palms, the monkeys and humming-birds, could not live when ice occupied the Amazon valley and the latitudes of Nicaragua. We can-

[8] Q. J. G. S., xxviii. p. 27.

not understand where they can have taken shelter, for it follows that, if the tropics were cold, the temperate regions must have been colder, and we have evidence, in the Brazilian caves, that precisely the same genera of animals occupied the country when they were filled as now, only with larger specific forms. This objection seems to be fatal, and Belt's attempt to find an explanation was very well met by Mr. Wallace, who says : " To get over the enormous difficulty as to what became of the exclusively tropical forms of insect and bird life that abound in such overpowering luxuriance in tropical America, he has recourse to the increased area of low land caused by the lowering of the ocean owing to the vast amount of water abstracted in the form of ice. But Mr. Andrew Murray's map of the 100 fathoms line of soundings shows that the tropical part of South America would not be materially increased in area by a depression of 600 feet, and another 600 feet would add proportionately less." [9]

In these views every zoologist and botanist known to me concurs, notably Burmeister, *facile princeps* in his knowledge of South American life, and Professor Orton. " If there was a glacial period in the Amazon valley, undoubtedly," says the latter, " it had lateral branches descending the valleys of the Orinoco and Paraguay ; there is a close similarity of the formation in these valleys to the Amazonian clay, which has resulted, we think, from a contemporaneousness, if not identity of origin, and so low is the watershed, especially on the north, that the two river systems are joined by natural canals. The glaciation of the whole earth at the same time is absurd, on biological and hydrological grounds ; if, therefore, an equatorial ice period occurred before or after the ice period of the high latitudes, we must imagine the temperate regions converted by a change of climate into a conservatory for the rich and peculiar life in the tropics, which is an unwarrantable assumption." [1] This is surely very true. If they had no such place of refuge, what became of the tropical animals and plants ? were they all destroyed and recreated ? were the monkeys, the jaguars, the peccaries, and the animals whose remains are found in the caverns and in the pampas deposits entirely exterminated, and a fresh set exactly like them created in their

[9] Nature, ix. 220. [1] Id. 304.

place? The position is assuredly ridiculous. And, again, should we not have had some traces of an Arctic fauna and flora in the surface beds of the tropics? but they nowhere occur. To again quote Professor Orton: "The almost total absence of typical North American plants in the highlands of the West Indies and on the Andes of the Equator does not favour the theory of a glacial migration."

If the polar ice-caps overflowed into the tropical regions of the new world, why not of the old? Why don't we find similar traces to those claimed by Agassiz in Africa and Southern India? On his premises they ought to be there, but none such have ever been forthcoming thence.

Turning aside from the biological, let us examine the more purely physical conditions of the problem.

If the tropical glaciers were merely the extensions, the skirts, of the polar ice-caps, as urged by Agassiz and others, then, as Dr. Ricketts points out, we cannot account for the fact that, near Colorado city, situated at a height of 6000 feet, and in the direct route of the supposed ice-cap, there are most remarkable examples of subaerial erosion, massive triassic (?) sandstones 50 feet and upwards in height, weathered into the most fantastic shapes, some of which are isolated monoliths, among them a magnificent natural obelisk, 250 feet high, called the "Cathedral Rock," situated in "the Garden of the Gods," Colorado; whilst in rocks of later formation are numerous peculiar columns in "Monument Park." Any of these would have been broken away by the pressure of an accumulation of snow persisting during comparatively a few years.[2]

The next argument I would use is an *à priori* one. It has been well stated by Dr. Ricketts and Professor Orton. "If by any possibility," says the former, "such a climate could have existed in equatorial regions, there must necessarily at the same time have been a period of most intense cold during winter in higher latitudes; but, with such conditions, the atmosphere could not be supplied with watery vapour, as it now is directly from equatorial waters, so that there would be but little contained in it to be precipitated in more northern climes, and therefore glaciers could not exist there. It would be as impossible for there to have been the system of glaciers

[2] Proceedings Geol. Soc. 1876-7, 7.

which *are known* to have existed in North America and in Europe as it would be for a steam engine to be set in motion without a fire underneath the boiler." [3]

Professor Orton says: "At the equator there is little variation of temperature. The snow-line on the equatorial mountains is therefore stationary, while the oscillation from a summer heat to winter cold in northern latitudes gives rise to a variable snow-line. Now, the snow-line at the equator remains throughout the year at about 15,800 feet . . . . to bring it down to the sea-level would require excessive cold. But this more than polar reduction of temperature, and the uniform climate, would destroy the conditions necessary for the manufacture of the glacier, which must be constantly fed; and the supply depends on an abundant snowfall, and this, again, on humidity. But an intense, unchanging winter would be a dry one. Besides, if a snow-field does not attain a temperature higher than zero, it can never become a glacier; for the particles are as incoherent as sand." [4]

Elsewhere the same writer urges that, " if the glacier were formed, it is difficult to see how it could move, for this depends on repeated accumulations of snow and ice at the high sources, and on a change of seasons. The Alpine glaciers move twice as fast in summer as winter, nor would the slope suffice, for, between Pébas and Pará, a distance of 1600 miles, the slope is only 8′ 5″, or about 2½ inches per mile, and from the tip-top of the Andes to the Atlantic the inclination is 6′ 30″. We conclude, therefore, he says, that if a sheet of ice ever spread from Cotopaxi to the mouth of the Amazon, it remained there as immovable as the mountains. Suppose the ice-sheet formed and moving, what would be its flow? Even if its rate equalled that of the Mer de Glace, a boulder from the Andes would be over 20,000 years in reaching the Atlantic; but when we consider its feeble slope, and its retardation by the constant trade winds, we may wonder if it ever completed its journey. Yet this Agassiz glacier is represented as doing a greater amount of work than the high-latitude glaciers, grinding up and covering the vast basin with 800 feet of detritus, ' the

[3] The Conditions existing during the Glacial Period, Livp. Geol. Soc. 1876-7, 7 and 8.
[4] Annals and Mag. Nat. Hist. 4th ser. viii. 302.

most colossal drift formation known,' and again, all the slope of any consequence lies between the axis of the Andes and Pébas, a distance of 450 miles. In this abrupt descent, 35 feet per mile, it must receive enough momentum to carry it over an almost level plain of 1600 miles." [5]

Turning from these *à priori* arguments to more directly inductive ones, it is a remarkable fact that Professor Hartt, one of Agassiz's companions, and one also of his champions, should have changed his opinions very materially, for we find him in 1871, writing in Silliman's Journal, that he had visited the district of Ereré in the previous year, and traversed it on foot in various directions. He speaks of a low ridge of trap there as having given rise to a great number of boulders of decomposition, which lie on the surface, or are buried in decomposed rock. " They have been carried down the beds of torrents," but they are nowhere erratics. To speak broadly, I did not see either at Ereré or in any part of the Amazonas anything that would suggest glacial action. " In regard to the surface clays of the Amazonian valley which Agassiz pronounced to be drift, he says they belong to several distinct epochs. Variegated clays of exactly the same appearance and structure, because formed of materials reworked or drawn from the same source, occur in the Devonian, Carboniferous, Tertiary, and in half a dozen successive formations of recent times." [6]

In another paper on the tertiary beds of the Maranon, we read, " I have shown that the supposed facts on which Professor Agassiz founded his theory, viz., the assumed identity of structure of the Serras of Ereré and Pará (Almeyrim), the occurrences of erratics of diorite at Ereré, etc., were no facts at all. Ereré is a monoclinal ridge of sandstone which no geologist would ever think of calling drift, and the supposed drift clays at its base contain lower Devonian trilobites, and are traversed by trap dykes; the supposed erratics of diorite are *boulders of decomposition ;* the serras of Pará are composed of horizontal beds of soft rocks undoubtedly more modern than the Serra of Ereré, and offering not the first evidence of glacial origin; the gigantic moraine which Professor Agassiz thought to have extended across the mouth of the Amazon

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[5] Op. cit. 302-303. [6] Silliman's Journal, 3rd ser. i. 294-5.

does not exist. Moreover, I have failed in finding, during
many months of careful search, anything like drift in the pro-
vince of Pará, and therefore, having no evidence whatever of
the former existence of glaciers in the Amazonas, the question
of the glacial origin of the valley need not be raised."[7]   While
thus emphatically sweeping away the postulated Brazilian
ice-sheet, Professor Hartt maintained the belief that local
glaciers once existed in the central and southern portions of
the Brazilian plateau, and says that Professor O. H. St. John
found similar traces in Minas Gereas, while these phenomena
were not visible in Piauhy and Maranon.

Professor Orton says : " The theory is short of positive
proof when we read the most unquestionable evidence. The
confession is made that the direct traces of glaciers as seen in
other countries are wanting in Brazil." There is not a trace
of furrows, striæ, or polished surfaces. The answer that the
rocks are so soft and pliable as to render their discovery hope-
less is not satisfactory. The granitoid rocks which border
the valley, and the schists and porphyries on the slope of
the Andes, ought to preserve some marks of the glaciation.
The pot-holes in the gneiss plains of Bahia are " exceedingly
well preserved, and have smooth sides," while all the plough-
ings and planings of the gigantic glacier over the same rock
have been utterly erased by disintegration ! The stone
structures of Brazil endure remarkably well, while the granite
of Quebec exfoliates so rapidly in winter that oil is used to
protect the buildings ; yet there is no lack of striæ in Canada.

Boulders occur only along the eastern region ; none have
been observed in the great interior basin. This is a strange
inversion : if a continental glacier moved down the Andes to
the Atlantic, we would naturally look for porphyritic boulders
scattered over the valley, and dwindling in number and size
as we near Pará. We are suspicious also that these so-called
boulders have not travelled. The only genuine erratics seen
by Professor Agassiz were found on the northern flank of
Ereré. All the others turn out to be " boulders of decompo-
sition." The boulders of Tijuca, in the Rio province, described
by Hartt, were not far-fetched ; the majority are of gneiss on
gneiss. The Ereré erratics are hornblendic and without

[7] Op. cit. iv. 53-58.

scratches. In regard to the great moraine which, according
to Agassiz, closed up the mouth of the valley, Dr. Newberry
says a moraine can hardly be formed by a glacier, except
when there are cliffs and pinnacles along its course. Inasmuch
as no traces of this moraine exist on the land, it has been
found convenient to say that this morainic wall must be looked
for in the depths of the Atlantic. . . . Thus we are called upon
to believe in the existence of a tropical glacier, 2000 miles in
length, moving for "hundreds of thousands of years" over the
continent, upon evidence which is singularly defective.

Agassiz declared that tertiary deposits have never oc-
curred in any part of the Amazonian basin; hence his con-
clusion that the variegated clays are a form of drift. Mr.
Orton and his companions found numerous shells imbedded
in the clays at Pébas, at the confluence of the Ambyacas
with the Maranon. These were examined by Mr. Gabb of
Philadelphia. Another deposit of similar shells was found
by Mr. Hauxwell thirty miles below Pébas and in other
places. Seventeen species were collected, all extinct, be-
longing to nine genera, only three of which have living
representatives. "The beds," says Mr. Orton, "can-
not, therefore, be later than the Pliocene." Many of the
species such as *Isaea lintea*, *Liris laqueata*, and *Dyris gracilis*,
are exceedingly delicate yet perfect. But Agassiz says the
Andean glacier must have ploughed the valley bottom over
and over again, grinding all the materials beneath it into
a fine powder. "How did these shells escape during the
kneading process the drift has undergone beneath the
gigantic ice-plough?" The supposition that they may
have been washed in from another locality must be rejected,
for they are plainly in place and not water-worn. Professor
Conrad assigns the shells to a tertiary and perhaps miocene
age, and says: "The species indicate fresh or brackish
water life, and the perfect preservation of the most delicate
parts, some specimens retaining even the epidermis, shows a
quiet lake or estuary. There certainly are no indications
of a ' grinding glacier.' "[8]

" It seems clear," says the same writer, " that they were
not transported from a distance, but lived and died in the

[8] Annals and Mag. of Nat. Hist. 4th ser. vii. 64.

vicinity of the spot in which they are found. The shells are filled with the same bluish or drab sandy clay, 'holding minute scales of mica and frequently ferruginous,' in which they occur." It is quite clear, continues Professor Orton, that the drift theory of this formation must be abandoned.

In 1875 M. Crevaux communicated a memoir to the French Geological Society, entitled "Faux blocs erratiques de la Plata." In this he tells us how when Agassiz professed to have discovered traces of a glacial age in the Amazon valley, he went on to say that these traces ought to be more obvious on the River Plate, which is so much nearer the pole, and he accordingly professed to find erratic blocks in the environs of the Cerra of Montevideo. M. Crevaux, having frequently examined the plains on which these rounded blocks occur, was quite convinced that ice had nothing to do with rounding and polishing them, but that their contour was due to running water. All the blocks, he says, are really *in situ* and not erratics at all.

A large collection of these blocks, some with rounded and some with oval contours, occurs at La Independencia, north of Montevideo. They vary in size from 4 to 400 cubic metres, have their angles rubbed, and are many of them rounded, and at a distance look as if they had been rolled to their present position, but in fact many of them are still attached to the ground. One of them, remarkable for its oval contour, is polished all over, and rests on a quadrangular block, like a monument on its pedestal. This, says M. Crevaux, is at first sight one of the most obvious of erratics, but on examination it was found to be attached to the block on which it rested, which, like it, was syenite. This rock was therefore polished *in situ*. Like all the rest, it was marked by a complete absence of striations.

At a distance of a kilometre from La Independencia are a number of hills with rounded contours. They are formed of granite and have marks like furrows upon them. On the top of one of these hills may be seen, dispersed over a distance of 300 or 400 metres, a great accumulation of blocks arranged in lines. They are all rounded and polished, and at a distance look just like a moraine. But

on examination they are found to be attached to the rock, and their contour is clearly shown to be due to foliation and the erosion of a soft, fine-grained granite. They are therefore anything but erratics, while, as to the fissures on which M. Honoré has so largely insisted as proofs of a glacial age, I will quote M. Crevaux's own words : " Ils occupent les deux flancs des collines, se correspondent dans leurs parties déclives et ne se voient jamais sur la cretê ; ils forment ainsi des sortes de terrasses qui ressemblent à toutes celles creusées par les eaux dans les terrains friables." In regard to the fact mentioned by M. Honoré, that the blocks are of different stones, he says this is so ; they consist partly of granite and partly of syenite ; and this is explained when we find that the subjacent granite is traversed by veins of syenite. M. Crevaux shows that the polish and rounding of the stones are due entirely to the torrential waters which here abound, and that they differ entirely from the results of glacial work. He concludes that, in all the places he visited, the rounded and polished rocks had the same appearance, were without striations and were *in situ :* " L'action des glaces doivant être écartée," he says ; " leur usure, leur polissage, s'expliquent parfaitement par le frottement exercé par des eaux très mouvementées, chargées de gravier et de sables, comme celles qu'on peut encore observer à Las Piedras." [9]

This concurrence of testimony, based upon careful observations, seems entirely to dissipate the conclusions of Agassiz as to the glaciation of Brazil and the tropics of South America.

In regard to Mr. Belt's reports, we labour under the difficulty that no trained geologist has been over the ground.

It must next be remarked that the boulders described by him, not only had no striæ on them, as he acknowledges, but were purely local and had not come from a considerable distance. At the most they had been transported a few miles, and were found in valleys where the rivers still flow. The evidence which they furnish may be compared with that from other places ; thus Schomberg long ago described, not

[9] Bull. Geol. Soc. de France, 3rd ser. iv. 304-308.

merely local, but far-transported crystalline rocks, which he
found scattered on the sedimentary deposits of Guiana, " a
region," says Murchison, " on which no glaciers could ever
have existed." [1]

Similarly, De La Beche described not only boulders, but
great masses of detrital gravel, as occurring in Jamaica and
derived from the neighbouring mountains, of St. Andrews
and Port Royal. These mountains, he says, are composed
of white limestone, porphyry, greenstone, red porphyritic
conglomerate, and silicious sandstone, with red sandstones
and conglomerates of older date. Rounded pieces of all
these rocks are found in the gravel of Liguanea ; most of
them are pebbles of no great size, but considerable boulders
both of greenstone and silicious sandstone occur. In the
lowlands of St. Catherine's and St. Dorothy's similar gravels
occur with sands and clays. In the plain of Vere and Lower
Clarendon he found similar beds, the stones being detritus
from the St. John's and Clarendon Mountains. De La Beche
never dreams of invoking a glacial period in Jamaica to explain
this detritus. Chardin, the Persian traveller, found great
boulders different from any stone in the country, and which
would take eight men to carry them, on the plains of Media,
and found no similiar stone *in situ* within eighteen leagues.

Mr. W. T. Blandford, in describing thegr avels of Persia
expressly mentions finding great blocks among them, which,
he says, cannot be attributed to ice action, but to that of
water. Similarly the well-known blocks found in the upper
Indus valley simulate, but are clearly distinguishable from,
true glacial boulders. Mr. Wynne, of the Indian Geological
Survey, altogether disputes the fact that they were dis-
tributed by glaciers. He says : " The Indus is famous for its
debacles, and these, carrying in their rush fragments of glaciers
laden with rock masses from the high regions, it appears to
me, would account very simply for the transport southwards
of the blocks referred to and also for their distribution. To
anyone familiar with the appearance of 'the Drift' of
Ireland and the glacial features of many of its hill regions,
the great difference between these features and the supposed
glacier work in the upper Punjaub, is so palpable that the

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[1] See Russia and the Ural Mountains, i. 551.

strongest possible doubts suggest themselves as to the pro-
bability of extensive glaciation at low levels in the North
of India." [2]

These are all cases of boulders occurring in low latitudes
and similar to those described by Mr. Belt, which have been
examined and described by skilled geologists, none of whom
have countenanced the notion that they prove a condition
of tropical glaciation, but rather that they are the results of
abnormal torrents, and to this cause I would attribute the
dispersion of the unstriated and local boulders and detritus
described by Mr. Belt.

Having shown that the tropical glaciation supported by
some authors is not attested by any satisfactory evidence,
let us now turn to the so-called polar ice-caps, and first let
us test the theory on *à priori* grounds. I will begin by
quoting from that very ingenious and logical writer Reclus,
whose work I have often laid under contribution. In speak-
ing of the foci of greatest cold on the earth, he says it is very
probable that in the polar circle there exist at least two
isothermal islands of cold corresponding to the two isothermal
islands of heat which are found in the neighbourhood of the
equator. According to Brewster, there are in the Frozen
Ocean of the north two of these regions of greatest cold, real
meteorological poles moving incessantly according to the
alternation of the seasons, but in all their oscillations keep-
ing themselves at several hundred miles' distance from the
geometrical pole. One of these poles of cold is found to the
north of the Asiatic continent not far from the Archipelago,
known under the name of New Siberia, and its average tem-
perature is about 1·5°. The American pole oscillates in the
midst of the western islands of the polar Archipelago, and
the average there is more than 2° below zero. The researches
of Maury have rendered it very probable that in the Antarctic
hemisphere there also exist two poles of cold. Reclus then
proceeds: "The regions whose climate is most severe would
thus be situated under latitudes which man has already
visited, and consequently the poles properly so-called would
not be that formidable citadel of ice that geographers once
imagined. *It is erroneous to believe in the existence of an ice*

² Geol. Mag. 1881, 97.

*field all round the pole gradually thickening towards the centre
and erroneous to picture to oneself, the two extremities of the
terrestrial axis as for ever unapproachable, because of the intensity
of the cold.*   Besides, the calculations of the mathematician
Plana tend to make us believe that the total quantity of heat
received increases gradually from the polar circle to the
central depression of the Arctic zone. . . . It is found that
the mean temperature, having gradually decreased from
the tropics to the limits of the glacial zone, afterwards rises
in a normal way as far as the pole, which would thus be, at
least theoretically, the warmest point of all the polar region,
the cold being less severe at the north pole than it is on the
coast of North America and Siberia at 1600 miles further to
the south.   However this may be, it is certain that during the
six months of summer the insolation is greater at the pole
than at any other part of the Arctic zone, for, according to
the expression of M. Gustave Lambert, " it is always noon,"
during the summer of the pole, because of the position of
the earth relatively to the sun.   According to calculations
made by Halley, nearly two centuries ago, the summer mean
must increase from the 60° of latitude to the north pole in
the proportion of 9° to 10°.[3]

Let us examine a little more closely the meteorological
consequences of this somewhat unexpected result, namely, that
the temperature of the polar circle increases as we near the pole.

It is a very elementary fact in meteorology that as we
ascend in the air we gradually rise through a stratum in
which clouds largely prevail and presently reach one where
they become infrequent.   It is not in the upper layers of the
atmosphere that clouds chiefly abound, but a good deal lower
down.   Clouds, to use the picturesque phrase of Tyndall, are
the visible summits of ascending columns of vapour rising
into the transparent atmosphere.   Directly such columns reach
a stratum of air cool enough to condense them, clouds are
formed.   It thus happens that the mean height of the zone
of clouds, which varies in different latitudes, is from $1\frac{1}{2}$ to 2
miles in Western Europe.

As a matter of fact, air in rising from a warmer to a colder
stratum must part with its moisture, which condenses, and

[3] Reclus, The Ocean, ii. 80-81.

falls. When it reaches a stratum where the cold is always below the freezing point of water it condenses, not into clouds formed of drops of water, but into clouds formed of particles of ice. This has been proved experimentally. The small white clouds called cirri, whose height according to Känitz is not less than four miles, have been shown by the luminous phenomena of reflection and refraction which they display, to be formed of minute particles of ice.[4]

The cold regions of the higher atmosphere, says Reclus, contain only a very small proportion of misty vapour; and the scanty flakes of snow which would fall on summits 45,000 to 60,000 feet high (if any such existed), would be soon swept away by the wind, or melted by the solar rays. On the sides of a mountain of this elevation there would be a belt of permanent snow, bounded on the lower side by a region of pasture ground, and on the upper by tracts of desert perfectly devoid of vegetation. According to Tschudi the quantity of snow which falls on that portion of the Alps which is above a height of about 10,800 feet is comparatively very small. Most of the clouds charged with snowflakes discharge their burden on the mountain slopes at elevations of from 7600 to 8600 feet.[5]

This is why mountains in Tibet are found absolutely denuded of snow, the dry winds which cross them instead of depositing snow having a tendency to evaporate what they find. Hence also why the snow is much lower on the northern flanks of the Himalayas than on the southern, where the heat is greater and the moisture more abundant.[6]

The reason for the fact here stated is clearly that damp air must, in passing into a cold region, part with its moisture. It is clear that it must do this equally, whether in rising it meets with colder air in ascending to a higher stratum, or in moving laterally it comes to a colder latitude. Those who have argued in favour of a great deposit of snow and of snow-formed ice at the poles seem to overlook not only the fact that the poles of cold are some distance from the actual poles, but that any damp winds moving north or south must, long before they reach either pole, pass over areas

[4] Reclus, The Ocean, ii. 10.   [5] Reclus, The Earth, i. 182.
[6] Id. 165-6.

whose temperature is so low that their moisture must be drained out of them. Thus, in moving northwards they would, about the 75th parallel, reach a point where the snow-line actually coincides with the sea level.

Nordenskiold noticed that on the 80th parallel of latitude the hills near the coast are bare of snow in summer, while the low ground near the sea is always encumbered with ice and snow. The same cause produces early abstraction of vapour, and may enable the poles to be free of ice. Any ice-cap must have been much thicker at the 70th parallel than further north.[7]

This view has been urged with his usual clearness by Lyell. " When," he says, " colder winds from the temperate zone first meet the frozen air of the Arctic or Antarctic regions, they will part with their moisture, so that the snow will increase on the outer margin of the Antarctic continent rather than in the interior, as it is well known that great falls of snow take place chiefly when the thermometer is about 32° F., and that little, if any, ever falls when the temperature is much lower."[8]

On the same subject Mr. A. Wallace says : " We have shown that the production of any such ice-cap (as that believed to exist at the Antarctic pole) is improbable, if not impossible, because snow and ice can only accumulate when precipitation is greater than melting and evaporation, and this is never the case except in areas exposed to the full influence of the vapour-bearing winds. The outer rim of the ice-sheet would inevitably exhaust the air of so much of its moisture that what reached the inner parts would produce far less snow than would be melted by the long hot days of summer." [9]

Dr. Ricketts says of the advocates of a polar ice-cap : " They overlook the effects of intense cold in precipitating moisture contained in the atmosphere. With a winter of such severity as occurs within the Antartic circle, the whole of the water would be removed before the winds could pass over a radius of 1400 miles. Certainly there would be no snow covering the ground within many degrees of the pole which would not be removed by the heat of the sun during the long days of summer." He quotes Captain Nares' reports that in the Arctic

[7] Geol. Mag. 1876, 91.   [8] Principles, i. 287.
[9] Island Life, 156.

expedition on June 13th and 31st, a thermometer, with a blackened bulb, *in vacuo*, exposed to the sun's rays, registered + 128° and + 129° Fahr. respectively ; the temperature of the earth's surface being at the time + 27° and of the air + 34°.[1]

I cannot think Dr. Croll has at all met these arguments. He quotes the accepted fact that a vast quantity of snow falls in the neighbourhood of the Antarctic continent in the high southern seas visited by Sir John Ross and other navigators. This fact no one disputes. Sir J. Hooker says : "The quantity of snow that falls in summer is enormous, south of latitude 50–60°. Certainly it fell on half the days of each summer month during the three seasons we spent in those seas, and I think in one month snow fell every day." [2] Again, Lieutenant Wilkes estimated the snow-fall of the Antarctic regions at about thirty feet per annum, and Sir John Ross says that, during a whole month, they had only three days free from snow.[3] This is, of course, in the latitude reached by those navigators, which was a long way from the south pole.

This is not only undisputed, but it seems to me to be the best evidence available in favour of the views above quoted from Lyell and Wallace. The very fact of such a prodigious snow-fall taking place in the Antarctic region at such a distance from the pole, is the best evidence that the winds blowing south are discharged of nearly all their moisture long before they reach the pole. Dr. Croll argues that, although the wind may be considerably drained, yet, that in passing from a low latitude to a high one, so much more air is compressed into a small compass, that the total quantity of moisture still left to deposit must be considerable. His words are : "There is one circumstance which must tend to make the snow-fall near the south pole considerable, and that is the inflow of moist winds in all directions towards it ; and, as the area on which these currents deposit their snow becomes less and less as the pole is reached, this must to a corresponding extent increase the quantity of snow falling on a given area. Let us assume, for example, that the clouds in passing from lat. 60° to lat. 80° deposit moisture sufficient to produce,

[1] Proceedings, Liv. Geol. Soc. 1876-7.
[2] Letter to Croll, see Discussions in Climatology, etc., 76 (note).
[3] Id.

say, thirty feet of snow per annum, and supposing that by the time they reach lat. 80° they are in possession of only one-tenth part of their original store of moisture, still, as the area between lat. 80° and the pole is but one-eighth of that between lat. 60° and 80°, this would, notwithstanding, give twenty-four feet as the annual amount of snow-fall between lat. 80° and the pole."

All this is assuredly based on a meteorological fallacy. I have argued in Chapter XI., that the area about the poles is too small to contain the air which flows towards them from the temperate regions. They cannot hold it, because there is no room. In order that they should do so, we ought to have a continuously increasing gale as we approach the poles, of which we have no evidence whatever; and it seems to me perfectly plain that the damp air from the temperate zone when it reaches the latitude of about 70°, where, as we have seen, it drops its enormous loads of moisture, gets immediately much warmer by the very fact of this condensation, and must rise, and when it rises it probably returns again to the temperate zone, forming a circulating current corresponding to that we have shown to exist in the tropics. The air about the poles, if it circulates at all, has, in all probability, a very local and partial circulation. This fact quite destroys Dr. Croll's theoretical contention.

His second argument does not seem to be any more conclusive. He urges that, " granting the ice does accumulate in a huge ring at some distance from the pole, this ice must flow away in two directions, outwardly and inwardly; one portion of it falling back towards the pole, and continuing to accumulate there till the resistance to the inward flow becomes greater than the resistance to the outward, a state which would not be reached till the ice became, at least, as thick on the poleward as on the outward side." [4]

This view is at issue with another ingenious view he puts forward in the same chapter, based upon the fact that the ice of the icebergs which have been found floating in the South Atlantic and Pacific, is formed of regularly stratified layers, of which the lower ones are a good deal thinner than the upper ones. This he explains by their

[4] Id. 80.

having come a longer distance, and having had to spread out, since they have travelled from where they have had to occupy a smaller, to where they occupy a larger area. His words are: "I have shown that this thinning of the strata from the top downwards, is a necessary physical consequence of continental ice radiating from a centre of dispersion. Assuming the south pole to be this centre, a layer, which in, say, lat. 85° covers one square foot of surface will, on reaching lat. 80°, cover two square feet; at lat. 70° it will occupy four square feet, and at lat. 60° the space covered will be six square feet. If the layer was one foot thick at lat. 80°, it will be three inches thick at lat. 70°, and two inches at lat. 60°. Had the square foot of ice come from lat. 89°, it would occupy thirty square feet by the time it reached lat. 60°, and its thickness would be reduced to $\frac{1}{30}$ of a foot or $\frac{2}{5}$ of an inch. Now the lower the layer the older it is, and the greater the distance which it has travelled. A layer near the bottom may have been travelling from the pole for the past 10,000 or 15,000 years, whereas a layer near the top may perhaps not be twenty years, and may not have travelled the distance of a mile. The ice at the bottom of a berg may have come from near the pole, whereas the ice at the top may not have travelled a hundred yards."

Whatever the value of this reasoning, it is at issue with the view that ice moves inwards as well as outwards, from any circumpolar ring of accumulating snow.

On *à priori* grounds, therefore, the postulate of ice-caps which Dr. Croll and others employ, seems inadmissible. Nor can we find much support for it in the actual facts of physical geography. Let us first consider the north polar area.

Reclus says: "The experience of polar navigators has fully confirmed the results of the theory according to which the series of Arctic isothermals mark a gradual increase of temperature. In his celebrated voyage of 1827, Parry ventured with his bold companions on the great ice-field which extended to the north of Spitzbergen. Imagining that this field was a real continent of ice, he hastened to cross these polar regions as if they were the frozen steppes of Siberia; but as the sledges advanced towards the north, the ice-field

became lighter and more fissured. It was floating to the south, carried along by a drifting current, and before the travellers, on the side next the pole they so much desired to approach, stretched an open sea, where floated only a few scattered pieces of ice. At the extreme point of his perilous expedition towards the north, Kane also discovered an immense sheet of water completely free from ice, and that immediately to the north of Smith's Strait, where the mingled fragments of glaciers and ice-fields form a labyrinth difficult to traverse. To the north of the coast of Siberia, all encumbered with 'torosses,' or hummocks, Wrangell and other navigators have also ascertained the existence of an open sea, to which they have given the name of Polhynia. Finally, in the Antarctic hemisphere, Sir James Ross found tracts, relatively free from ice, beyond that high wall through which he had to make his way with so much difficulty. Thus, it may be admitted as probable that there does not exist a region of unbroken ice at the two extremities of the earth, but rather a more or less open sea surrounded on all sides by islands."

Lyell says very truly, "Antecedently to experience, it might have been thought that the thickness of the ice would increase as it extended northwards, but Parry penetrated within about seven degrees, and Kane within five degrees, of the north pole; and they both of them found open sea there, though they had reached a latitude so much higher than that in which the continent of Greenland is enveloped in a winding sheet of perpetual snow. From such facts the geologist may learn that, although in the glacial epoch certain mountain-chains and adjoining lowlands may have been buried, in temperate latitudes, under a vast covering of ice, yet the waters of the ocean in much higher latitudes may not at the same time have been frozen." [5]

" It is a very remarkable and most suggestive fact," says Mr. A. Wallace, " that nowhere in the world at the present moment are there any extensive lowlands covered with perpetual snow. The tundras of Siberia and the barren grounds of North America are all clothed with some kind of summer vegetation, and it is only when there are lofty mountains or plateaux, as

[5] Lyell's Principles, i. 285-6.

in Greenland, Spitzbergen, and Grinnell Land, that glaciers, accompanied by perpetual snow, cover the country, and descend in places to the level of the sea."

Again, the same author says: "Even in the very highest latitudes reached by our last Arctic expedition, there is very little perpetual snow or ice, for Captain Nares tells us that north of Haye's Sound, in lat. 79° N., the mountains were remarkably free from ice-caps, while extensive tracts of land were free from snow during summer, and covered with rich vegetation, and an abundance of bright flowers. The reason of this is evidently the scanty snow-fall, which rendered it sometimes difficult to obtain enough to form shelter banks around the ship, and this was north of 80° N.L., when the sun was absent for 142 days." [6] Elsewhere he explains the fact of the glaciers on the northern part of Smith's Sound not reaching the sea as they do further south, as a consequence of the snow-fall there being less than the sun can melt, the snow-bearing clouds discharging their contents principally in latitudes further south, and the land ice being formed of un-dissolved snow.

In the land-locked sea reached by Nares' expedition, where the ice was evidently of great age, and which he called the Palæocrystic Sea, there was nothing corresponding to an ice-cap although the conditions were exactly suited to it.

"So far as we know," says Sir W. Thompson, "the north pole is under water. The farthest north that has yet been reached is by Captain Markham, who went 28 miles from the shore, and reached lat. 83° 20′ 26″ N. On boring the ice it was discovered to be only sixty-four inches thick, and they found water seventy-six fathoms beneath. This was within 399 miles of the pole. It was water twenty-eight miles from the shore. They went over *floating* ice, which was very hummocky. Drift timber affords evidence that there is great freedom for currents to flow under the ice across the polar region." [7]

No glaciers of any size exist westward of the 85° meridian of W. Longitude in Lancaster Sound, and no true icebergs are met with in the great Arctic American archipelago. Here

[6] Island Life, 130-131.
[7] Sir W. Thompson, Trans. Geol. Soc. Glasgow, viii. 330.

and there, in dark and sunless ravines of North Devon and Melville Island, a diminutive glacier may be found, but it never reaches the sea so as to make icebergs.[8]

" A very noticeable feature of Grinnell Land," says Colonel Fielden, " is the scarcity of glaciers, and the non-existence of an ice-cap, such as prevails in N. Greenland. In Grinnell Land N. of lat. 81°, no glaciers descend to the sea-level, which they do on the same parallel on the opposite Greenland coast of Hall Basin." [9]

Nordenskiold tells us that in summer the Siberian tundra is completely free of snow.[1] Again he says, " Along the whole coast, from the White Sea to Bering Straits, no glacier was seen. During autumn the Siberian coast is nearly free from ice and snow." [2] In passing up the Yenissei in 1875 in August, he only saw snow in one place, a deep valley cleft of some fathoms in extent.

The evidence of the Arctic area is, therefore, consistent and complete in favour of the view that there is nothing like an ice-cap present there now.

We have reason to believe this is also the case at the Antarctic pole. Unfortunately, we know little of the south pole, but that little points to a conclusion very different from that of Dr. Croll and others.

Dr. Croll's views on this subject are tied up with his notion that the Antarctic continent, instead of consisting of high land either in the form of plateaux or of ridges, is for the most part a flat, low surface, perhaps broken up into islands, and that upon it is accumulated a vast cap of ice, gradually rising in height towards the pole. The hypothetical basis to much of his reasoning is, as we shall show in Chapter XIV., without any foundation. Of course it is easy to postulate what we like of the unknown without fear of contradiction, and unfortunately the Antarctic continent is very largely indeed a *terra incognita*, and its actual structure is very largely disguised by the immense ice barrier which surrounds it and which must surround it under either hypothesis. What we actually know of it does not certainly favour the notion of its being a

[8] McClure's Journal, ed. by Capt. Sherard Osborne, 149.
[9] Q. J. G. S. xxxiv. 567.
[1] Voyage of the *Vega*, 378.   [2] Nature, November, 1879.

mere mass of ice resting either on very flat lands or on the sea bottom.

In the sense in which Croll and others postulate an ice-cap, that is to say, a dome of ice pressing outwards from the pole, and not in the nature of a glacier moving down from mountain lands, but along low ground or a level plain, there is not, so far as we know, any warrant for his conclusion.

This being the evidence of *the existing ice* at both poles, let us now turn to the evidence we have as to the condition of the two poles during the so-called Ice Age.

In regard to the southern hemisphere we have the fact already enlarged upon, that there is no evidence of an ice age in the southern hemisphere in Pleistocene times at all. This being so, it is more profitable to turn to the north polar area, and to test the theory there. First, because we know the country much better, it having been frequently visited and explored in various directions, and secondly, because there we have to deal with the typical phenomena which are supposed to evidence an ice age, which are conspicuously wanting in the opposite hemisphere, namely, the widespread glacial traces which spread over large areas in both the old world and the new, areas which are, apparently, united with the pole by continuous conditions. There, if anywhere, the evidence of an ice-cap, therefore, is valuable if it is forthcoming. There, however, it utterly fails.

There are two respects in which it fails, both of which it seems to me are absolutely fatal. In the first place the glacial phenomena are entirely absent from one half the circumpolar region, a condition of things quite inconsistent with an ice-cap culminating at the pole and pressing outwards, and secondly, the glacial striæ and the boulders which occur on the borders of the Arctic Sea in that half of the circumpolar region where glaciation is conspicuous can be shown to have moved from the south northwards, and not from the pole outwards. I cannot conceive two better tests of the whole question.

First, in regard to the distribution of the glacial phenomena. It is a familiar fact that there are no traces of glaciation in Northern Asia, but on the contrary there is the most complete

and consistent evidence that no such traces are to be found either on the flat tundras or on the higher ground. Murchison long ago showed that there are no marks of ancient glaciation on the Urals, which it must be remembered rise in places to a height of 1525 metres, and are in many places covered with snow for eight months in the year. Repeated visitors have tried in vain to find old glacial traces in the Altai mountains.

Lastly, traveller after traveller across Northern Asia speaks of the absence of all boulders, rounded rocks, etc., in Siberia from one end to the other.

Nordenskiold, who is a very skilful observer, tells us how he went over the north Asiatic continent in various directions trying to find traces of former Arctic conditions, where, if anywhere, they must have been most severe, and therefore have left the largest traces, but he found none. He says, *inter alia*, " It is certain the ice-cap did not extend over the plains of Siberia, where it can be proved that no ice age in a Scandinavian sense ever existed."[3]

Again he says, " Glacier ice-blocks occur abundantly on the coasts of Spitzbergen and north Novaya Zembla, but appear to be wanting or exceedingly rare along the whole north coast of Asia, between Yugor Schar and Wrangel Land. East of this they again occur, but not in any great numbers. This appears to show that the West Siberian Polar Sea is not surrounded by any glacial lands.[4]

Again he says, speaking of the Liachof Islands, " No traces of glaciers were visible, not even such small collections of ice as are to be found everywhere on Spitzbergen, where the land rises a few hundred feet above the surface of the sea. Nor, to judge by the appearance of the hills, have there been any glaciers in former times, and this is certainly the case on the mainland. The northernmost part of Asia in that case has never been covered by such an ice-sheet as is assumed by the supporters of a general ice age embracing the whole globe."[5]

This being the evidence from Northern Asia, let us now turn to America.

[3] *Voyage of the Vega*, ii. 246. [4] Id., i. 424.
[5] Id., i. 418.

Mr. Dall, who knows the country well, says, "The north-eastern portion of the United States was covered, as Greenland is now, with a nearly, if not quite, continuous glacier sheet. From the evidence before us we are unable to declare that this general ice sheet extended to Siberia, Alaska, or the entire West Coast of America." [6]

Elsewhere he says, "Three years' exploration, with a strong inclination to develop the facts of the case, failed to obtain on the shores of Norton Sound, or in the valley of the Yukon, any evidence whatever of such action.   Once only were polished rocks met with, and they proved on examination to be ' slicken sides,' while no instances of transported materials, scratches, boulders or moraines were anywhere met with. . . . Thomas Simpson especially remarks the absence of drift boulders on the Arctic coast, west of Return Reef of Franklin.   The most characteristic evidences of glacial action, which a child could not overlook, are quite absent in the valley of the Yukon." [7]

"Little is known," says Dr. Wright,[8] " of the vast region lying to the north of the lofty mountains forming the southern border of Alaska, but from the reports of Schwalka, Dall and others it has long seemed improbable that there was any southerly movement of ice over it, thus disproving the theory, somewhat prevalent, of a universal polar ice-cap." [9]

Colonel Fielden, at once a well-known Arctic explorer and naturalist, says : " We find, roughly speaking, that between the 40th meridian of east longitude, passing through the White Sea, and the 160th of west longitude, passing through Alaska but separated by the Atlantic Ocean, is included all that portion of the northern hemisphere which presents us with proofs of that abnormal extension of ice which we style the Glacial Epoch.   Great as this extension of the glacial episode was, it is only fair to point out that from the Kola peninsula in 40° east longitude to Bering Straits, the vast tundras of Siberia, extending through one hundred and fifty degrees of longitude, do not appear to present any traces of that abnormal ice episode which is so remarkably impressed on North-Western Europe and the north part of America." After

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[6] Dall's Alaska, 459.   [7] Id., 461-2.
[8] The Ice Age in North America, 161-2.   [9] Science, xi. 1888, 186.

quoting Nordenskiold's observations, he continues : "There is, consequently, no evidence for assuming that during the extension of the glacial episode in Scandinavia, Great Britain, and North America, the climate of Siberia differed very greatly from what it is now. Presumably it must have been warmer, for the land at that time supported vast herds of mammoths, rhinoceroses, and other large mammals whose remains are found in the tundras of Siberia, and the more northern New Siberian Islands." [1]

Canon Tristram, in his paper on the Polar Origin of Life, speaks in the same way. "The process of the glacial epoch," he says, "with the phenomena and results of which we are familiar, appears to have been confined to the region between Hudson Bay and the White Sea. There does not seem to be any clear evidence that there was any synchronous period of gelation, either in northern Asia or in America, west of Hudson Bay." And after quoting Geikie and Nordenskiold he continues : "All the evidence tending to show the limited area of the glacial epoch, proves that we have no need to invoke changes connected with the eccentricity of the earth's orbit, for then the extension of the ice-clad region would have been circumpolar, instead of being grouped round the North Atlantic." [2]

Mr. Campbell, writing from Salt Lake City, says : "There is nothing to indicate any glacial action at all between Chicago and Salt Lake. In rising from 726 feet above the sea to 8242 (the highest point on the railroad), and in descending to 4340 feet, I could see no single mark of ice action in the region. For 164° of longitude, about latitude N., from the Volga to this place I have found nothing to support the idea that solid ice extended from the pole to the equator. . . . I have found nothing to indicate an ice-cap in these latitudes thus far on my way round the world." [3]

While there is a singular absence of traces of glacial phenomena in one half of the circumpolar world, the curious and decisive fact remains that in the northern portion of the area where these traces occur, the evidence of the direction of the

[1] Address to the Norfolk and Norwich Naturalists' Soc., March 30th, 1886, 162-3.
[2] Id., 1887, 206-7.   [3] Q. J. G. S., xxx. 475-6.

striæ and the drift of the boulders is very strong against the polar area having been the source of the ice, i.e. against the notion of ice-caps.

The first person to produce direct evidence on the subject was Böhtlink, who, in a paper on his travels in Finmark published in the 7th volume of the Bulletin of the St. Petersburg Academy, pointed out that, while the striations and scratches at lakes Onega and Ladoga, in southern Finland, and on the Gulf of Bothnia, on the rocks between Gamla-Carleby and Tornea run in a north-west direction, and present their smooth sides to the north-west,—the opposite is the case on the coasts of the White Sea and the Icy Sea, for there the scratches run from south-west or west, and the eminences are polished on the sides facing those directions. This occurs on the coast of the Icy Sea, from the Varanger fiord to Swiatoi-noss. In a map these conclusions are marked by arrows.[4]

This fact has been amply confirmed by later explorers, and is admitted by Mr. James Geikie, who says : " The direction of the glaciation in the extreme north of Scandinavia, the peninsula of Kola, and north-eastern Finland demonstrates that the great Mer de Glace radiated outwards from the high grounds of Norway and Sweden, flowing north and north-east into the Arctic Ocean and east into the White Sea, and thus clearly proving that Northern Europe was not overflowed by a vast ice-cap creeping outwards from the north pole, as some geologists have supposed. I can see no evidence whatever that any European land was ever infringed upon by polar ice, much less overflowed by it."[5]

What is true of Northern Europe is true also of North America.

Dr. George M. Dawson, in a paper on the glaciation of British Columbia, says : " For the Arctic coast of the Continent (i.e. of America) and the Islands of the Archipelago off it, there is a considerable volume of evidence to show that the main direction of movement of erratics was northward. The most striking facts are those derived from Professor S. Haughton's appendix to M'Clintock's voyage, where the occurrence is described of boulders and pebbles from north Somerset at localities 100 and 135 miles north-eastward and

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[4] Ed. New Phil. Journal, xxxii. 103-4.   [5] Great Ice Age, 404-5.

north-westward from their supposed points of origin. Professor Haughton also states that the east side of King William's Land is strewn with boulders of gneiss like that of Montreal Island to the southward, and he points out the general northward ice movement thus indicated, referring the carriage of the boulders to the floating ice of the Glacial Period."

The copper said to be picked up in large masses by the Exquimaux near Princess Royal Island in Prince of Wales Strait, as well as on Prince of Wales Island, has likewise, in all probability, been derived from the copper-bearing rocks of the Coppermine River region to the south, as this metal can scarcely be supposed to occur in place in the region of horizontal limestone where it is found.

Dr. A. Armstrong, surgeon and naturalist to the *Investigator*, notes the occurrence of granitic and other crystalline rocks, not only on the south shore of Bering Land, but also on the hills at some distance from the shore. These, from what is now known of the region, must be supposed to have come from the continental land to the southward.

Dr. Bessels again remarks on the abundance of boulders on the shore of Smith's Sound, in latitude 81° 30′, which are manifestly derived from known localities on the Greenland coast much farther southward, and adds: "Drawing a conclusion from such observations, it becomes evident that the main line of the drift, indicating the direction of its motion, runs from south to north."

It may further be mentioned that Dr. R. Bell, of the Canadian Geological Survey, has found evidence of a northward or north-eastward movement of glacier ice in the northern part of Hudson Bay, with distinct indication of eastward glaciation in Hudson Strait. For the northern part of the Great Mackenzie Valley we are as yet without any very definite information, but Sir J. Richardson notes that Laurentian boulders are scattered westward over the nearly horizontal limestones of the district.[6]

Mr. Wright says the expedition of Dr. George M. Dawson to the headwaters of the Yukon, in the summer of 1887, seems to demonstrate that there was actually a movement of ice

[6] Geol. Mag. 1888, 349-50.

from the northern flanks of the mountains towards the interior. Thus he writes :

" In the Lewes and Pelly valleys, traces of the movement of heavy glacier ice in northward or north-westward directions, were observed in a number of cases, the grooving and furrowing being equally well marked at the water level and across the summits of hills several hundred feet higher. The facts are such as to lead to the belief that a more or less completely confluent glacier mass moved in a general north-westerly direction, from the mountainous districts south of the southern sources of the Yukon towards the less elevated country which borders the lower river within the limits of Alaska. This observation, taken in connection with the evidence of the former northward movement of glacier ice in the arctic regions to the east of the Mackenzie, appears to have very important bearings on theories of general glaciation." [7]

This evidence is assuredly complete ; I may add that it may be supplemented by a curious fact noticed by Nordenskiold, who says of the Bear Islands : " They are formed for the most part of plutonic rocks, whose upper part has weathered away, leaving gigantic isolated pillars. Four such pillars have given to the easternmost of the islands the name Lighthouse Island. Similar ruin-like formations are found not only on Cape Baranof, which lies right opposite, but also at a great number of places in that portion of the north coast of Siberia which lies farther to the east. Generally these cliff-ruins are collected together over considerable areas in groups or regular rows. They have thus when seen from the sea so bewildering a resemblance to the ruins of a gigantic city, which had once been surrounded by strong walls, and been full of temples and splendid buildings, that one is almost tempted to see in them memorials of a Tamerlane or Chinghiz Khan up here in the far north." [8]

These pillars and prominent unweathered rocks in the very high latitudes of Asia, are also mentioned by Baron Toll and Professor Bunge, in their recent expedition to the Bear Islands. As they lie in the very track of a great polar ice-cap, they must have been broken or worn or weathered away, if it ever existed.

[7] Science, xi. 1888, p. 156.   [8] Voyage of the *Vega*, i. 428.

In every way, therefore, we can test the question, the evidence for the former existence of polar ice-caps fails, and they have hardly any supporters left in England. Professor Ramsay, who was a staunch supporter of extreme glacial views, would have none of them, while Mr. J. Geikie, as we have seen, also discards them. I confess to being puzzled, however, by the attitude adopted by the latter writer. I cannot understand how he can adhere to the astronomical theories about the cause of the glacial age, of which he is a champion, and discard the theory of ice-caps. The notion of ice-caps and that of glaciation being due to varying eccentricity, or obliquity of the orbit, directly or indirectly, seem wedded together, as Croll no doubt thought, and it seems to me illogical to maintain one view and discard the other. On this subject I agree with Colonel Fielden, who says: "If we are correct in assuming that one half of the northern terrestrial hemisphere shows no trace of the extension of the glacial ice-cap—and evidence at our command appears to be conclusive on this point—whilst the other half does, we can hardly rely on the theory, that changes in the eccentricity of the earth's orbit, even with the indirect physical agencies thereby engendered, could account for the glacial epoch in North-Western Europe and North America. If the glacial epoch was due to incidences connected with the eccentricity of the earth's orbit, surely the extension of the glaciated area in the northern hemisphere would have been circumpolar, and not semi-spherical." [9]

In every way the position is tested, it seems to me, the theory of ice-caps utterly fails, and I will conclude this chapter with the views of two experienced geologists, with whose opinions on the subject I completely agree.

President Chamberlin says: "The more we know and ponder upon the enormous development of ice upon the plains of North-Eastern America, and contrast it with the relatively feeble development and dispersion from the mountainous regions of Alaska, which now bear the greatest glaciers outside of the arctic regions, and the relative absence of such accumulations to North-Eastern Asia—in short, the more we consider the asymmetry of the ice distribution in

[9] Address to the Norfolk and Norwich Naturalists' Society, March 30th, 1886, 162-3.

latitude or longitude and its disparity in elevation, the more difficult it becomes to explain the phenomena upon any astronomical basis, correlated though it be with oceanic and aerial currents, and geographical features, by whatsoever of ingenuity." [1]

Professor Judd says : " In the face of all these facts it is impossible to avoid the conclusion that the so-called 'glacial epoch' was a purely local phenomenon, confined to Western Europe and Eastern America. . . . Hence we are altogether spared the necessity of discussing such hypothetical explanations for secular changes of climate as have been advanced by Croll, Belt, Drayson and others, seeing that, not only are there no facts calling for the aid of such hypotheses, but that the conclusions of both the palæontologist and field geologist point in quite a different direction. We are far from regretting the publication of those charming works which have lately treated geological questions on the hypothesis of the occurrence of a rhythmical succession of hot and cold periods during the earth's past history. Yet we cannot help thinking that, had their authors paid a little more regard to the established results of palæontological investigation, each of the works in question might have been presented to the public in the form of three thin volumes, rather than as a single bulky one. " [2] In this conclusion I most completely concur, and it states very tersely the view for which I am contending.

Having reduced the problem to be solved to more modest dimensions, we now have to face the theories generally advanced to account for the glaciation of North-Eastern America and North-Western Europe. These theories involve the postulate of " Ice-sheets " acting as great erosive agents, and moving independently of the general contour of the country, and therefore of gravity. Before we can deal adequately with these subjects, we must consider the cause and process of glacier motion.

[1] Proceedings Amer. Assoc. of Science, xxxv. 211.
Geol. Mag. 1876, 535-6.

CHAPTER XIII.

THE CAUSE OF GLACIER MOTION.

" Ye ice falls, ye that from the mountain's brow,
　Adown enormous ravines, slope amain ;
　Torrents, methinks, that heard a mighty voice
　And stopped at once, amidst the maddest plunge.
　Motionless torrents, silent cataracts."

　　　　　　　　　　　　　　　　　COLERIDGE.

The vicissitudes in the theory of glacier motion—The nature of glacier
　　ice, its granular structure—Evidence of the movement of glaciers
　　—The infiltration theory—Scheuchzer—T. and J. de Charpentier,
　　Agassiz—Forbes' refutation of it—Heim, Bonney—Granular move-
　　ment theory—Hugi, Grad, Forel, Hagenbach—Moseley's dilata-
　　tion and contraction theory—Its criticism by Croll, Blake, Forbes,
　　Matthews, and J. Ball—Its revival by Brown—The fluidity due to
　　pressure-theory—Sutcliffe, J. Thompson—Its criticism by Brown and
　　Tyndall—Croll's molecular theory—Its criticism by J. Burns—Blake
　　and Heim—The sliding theory—Gruner, Deluc, De Saussure, Hopkins,
　　Mallet, Martins—Criticism of this theory—Forbes and Matthews—
　　The viscous theory—Bordier—Basil Hall—Rendu—Forbes—Polemic
　　between Forbes and his opponents—Agassiz. Moseley, Tyndall—
　　Crushing and regelation theory—Christie, Tyndall—Criticism of
　　this theory—Bonney, Huxley, J. Ball—Resuscitation of Forbes'
　　theory—M. Williams, Kane, Hayes, J. Ball, Matthews, Bianconi,
　　Aitken, Pfaff, Trotter, Mann—McConnell and Kidd—Andrews—
　　General Conclusion.

THE theory of glacier motion has involved a long and difficult
polemic, in which nearly all the distinguished physicists of the
last century have taken part, and in the course of which many
hypotheses have been forthcoming and been in turn dis-
carded. In this it shares the fate of many other theories.
Where it differs from the rest is in that, whereas at one time
opinion gradually converged upon one explanation, namely,
that of Forbes, as alone meeting the facts, that explanation
was in turn sharply challenged on empirical and *à priori*
grounds, and for many years it had to give place to other
theories which seemed more plausible. Quite recently again,
more accurate and careful experiments have shown that Forbes

was right. This fact has scarcely yet permeated scientific opinion, and is certainly hardly appreciated, although it is of the first moment in settling questions of far-reaching importance in general physics, and especially in theoretical geology.

As we rise in the atmosphere the cold increases and we speedily reach a height when the temperature is always below the freezing point of water all the year round. It is clear that above this line no rain falls, but only snow, and further that this snow when it falls remains in the condition of snow, and does not melt, except a thin superficial layer due to the influence of the direct rays of the sun, which is very slight. Above this line, then, not only does the snow remain as snow, but also as dry snow, and in a loose, powdery condition, which no ordinary pressure will alter the structure of or convert into ice. This is the case on the higher Himalayas and the Andes. Below this line the temperature is at some seasons of the year above the freezing point of water, and the snow is consequently more or less melted annually, and is also in a more or less damp and moist condition. This line therefore marks an important frontier in the meteorological features of mountain lands.

Let us now turn to another such frontier, namely, the line separating the zone where all the snow which annually falls is melted away and that in which only a portion of it is melted; this is known as the snow line. This line varies in height according to the latitude. At the equator it is about 16,000 feet above the level of the sea, and it sinks to about the sea level near the poles. Above the snow line and below the line where the temperature is always under the freezing point of water, a certain portion of the snow which falls annually is melted by the summer heat, while another portion gradually augments in thickness from the snow line upwards. Tyndall describes the process in his usual graphic manner: "The sun first raises the superficial snow to 32° and then melts it. The water thus formed percolates the cold mass below and expels the air entangled in the snow, the liquid trickles down and gets frozen on to the granules which it meets with colder than itself, augments them in size, and cements them together." By this process, assisted by the consolidating influence of pressure, there is formed a mass of white, opaque,

frozen and consolidated, half snow half ice, the whiteness and opacity being due to the myriad air bubbles which it encloses.

This white opaque mass is what the French call *névé* and the Germans *Firn*. Its superficial layers are more snowy and white, and consist of nearly pure snow, while the deeper ones have more colour and consistence, and break on the larger scale into vast fragments, which are called seracs. The upper part of the *névé* is stratified, each stratum representing a considerable distinct snow-fall, but as we pass down into the more condensed and more solid ice, these signs of stratification disappear, and it assumes a homogeneous and more or less granular consistency.

Gradually the *névé* passes, as we descend into true glacier ice, blue in colour, and close and transparent in texture.

It is important to remember that glacier ice is, in internal structure, very different to ordinary ice made by freezing water in a pond or laboratory. It is formed out of granulated *névé*, and it never loses the characteristic of granulation.[1] As Messrs. McConnel and Kidd say in their recent paper, " Glacier ice is a sort of conglomerate formed of glacier grains (*Gletscherkörner*), differing, however, from a conglomerate proper in that there is no matrix, the grains fitting each other perfectly. In the winter, at any rate, the ice on the sides of the glacier caves looks quite homogeneous. But, when a piece is broken off and exposed to the sun's rays, the different grains become visible to the naked eye, being separated probably by thin films of water. Though the optical structure of each grain is found under the polariscope to be perfectly uniform, the bounding surfaces are utterly irregular, and are generally curved. The optic axes too of neighbouring grains seem arranged quite at random."[2]

M. Forel succeeded in imitating glacier ice by alternately allowing snow to freeze and pouring over it water above 0° centigrade in temperature. In this way, ice of the granular texture of glacier ice was produced, and it apparently follows that a glacier is formed by the periodical melting of its surface by the sun, rain, etc., and its subsequent freezing, a process

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[1] This fact was first published by Hugi in his well-known work Die Gletscher, 1843, p. 10.
[2] Proceedings Royal Society, xliv. 333-4.

assisted by the presence of the two containing walls of the valley in which it lies.

A glacier, then, consists of a solid mass of frozen water, the upper part of loose snow, the middle of semi-consolidated ice, and the lower of ice, properly so-called, which mass is either embowelled in a single mountain valley, or formed of several converging portions, filling several radiating subsidiary valleys, and uniting in one mass in the main depression.

The fact that a glacier is not stationary, but moves in its bed, must have been known at a very early date to the mountaineers of Switzerland. They were witnesses of the gradual progression of its lower part, called the ice foot, which in many cases has overwhelmed meadows and fields and even houses. They must also have noticed the gradual movement of the great masses of stone on the glacier's back, which could be seen year after year to alter their position relatively to the sides. This homely evidence must have made it plain in very early times to the Swiss shepherds and hunters, that glaciers are not reservoirs of stationary ice, but are rather frozen streams in motion. Facts of a more dramatic kind must also have occurred similar to those named by more recent travellers. Thus :—

Toussaint de Charpentier tells us how he was assured by Jacques Balmat, a native of Chamounix, when he was travelling there in 1818, that once in the summer months the Savoyard peasants went with their sheep to graze on a kind of oasis on the Mer de Glace known as " le Jardin," when one of these animals fell into a crevasse and was killed. Some years afterwards the animal came to the surface, *some distance down the valley*, and the flesh had been preserved quite fresh.

In 1827 Hugi built himself a hut close to a rock at the divergence of the Lauteraar and Finstnaar glaciers. In 1830 he found that it had moved 100 feet, in 1836 2420 feet, and in 1839 more than 4000 feet.[4]

In 1832 Forbes discovered, near the Moulins, portions of a ladder which De Saussure had lost on the Aiguille de la Noire, in the year 1788. The distance it had travelled in the

[3] Naturwissenschaftlicher Anzeiger der All. Schweiz. Gesell. für die Ges. Naturwiss. for 1821, p. 78.
[4] Mems. Helvetic Soc. of Naturalists, 1841, p. 185.

44 years was about 16,500 feet, giving an average of 375 feet per annum as the mean rate of progression.[5]

On September 25th, 1842, the same traveller lost a hammer, which fell into the great Moulin, opposite the ice cascade, du Talèfre. This hammer was recovered on June 22nd, 1858, "not far below the Tacul." [6] On the 29th of July, 1836, the guide Michel Dévouasson lost a knapsack on the Glacier du Talèfre, in a crevasse into which he had fallen. Fragments of this knapsack were found on the Glacier du Lechaud on the 24th of July, 1846, at a distance of 4300 feet from where it had been lost, which showed an annual progression of 430 feet.[7]

The most dramatic of these discoveries remains to be told. It shows what a long time a glacier continues its march. In 1885 there was found on the Brul side of the Theodul, about 80 metres from the end of the glacier, a plank and a shoe lying on the snow. Further digging disclosed two skeletons, the heads and hoofs of two horses, the latter with the iron shoes still on, a silver cross, two medals, one with the name Mathews on it, some beads of a rosary, some cherry stones, a bracelet with the initials P.M.R. or P.M.V., some fragments of clothing, twelve long boards, twelve bits of rope, one dated 1582, the others with initials. Sig. Vaccarone conjectured the remains were those of men flying from the plague, or the religious persecutors at Aosta, or of Spanish soldiers beaten back from Gressoney, by the men of Val d'Aosta or two sentinels in a cabin built as a guard-house.[8]

While facts like these must have made the motion of glaciers well known to the Swiss peasants from early times, it was apparently first published to the scientific world by Simmler in his work "De Alpibus," published in the middle of the sixteenth century.

When it was established that glaciers actually move, men began to try and find an adequate explanation of their movement. The various theories which have been suggested all appeal to one of two forces, namely, heat or gravity. We will examine them in turn, and in doing so shall find it convenient

[5] Travels, 87.   [6] Forbes's Life and Letters, 297.
[7] Thirteenth Letter on Glaciers, Ed. Phil. Journ. 1847.
[8] Alpine Journal, xii. 470.

not to follow a chronological order, but to first examine the various theories which more or less exclude gravity as a factor in glacier motion and which appeal to the action of heat in various ways.

In his so-called "Itinera per Helvetiæ Alpinas Regiones," 1723 (pp. 287-8), J. J. Scheuchzer, referring to the motion of glaciers, says : " The cause of this motion is not owing to any miracle, as those ignorant of physics suppose, but is to be ascribed to natural causes. The water flowing from the sides of the mountain on to the glacier enters its fissures and interstices, freezes again, and as it needs more space when thus frozen, as experiments have shown, it causes the glacier to thrust forward, and to carry with it sand and stones, some of them of great size," etc. He goes on to say that his opinion in the matter had been confirmed by observations made by two of his friends.

The theory thus hinted at was revived by Toussaint de Charpentier and Canon Biselx in 1819, and was published in " The Transactions of the Swiss Natural History Society " for 1821, p. 77, where it is stated that rainwater as well as water from the melted snow finds its way into the cracks and cavities of the ice-mass. The water which thus percolates freezes again, swells out, and causes the ice to split and to move. The theory was worked out in detail by T. de Charpentier's more famous brother, J. de Charpentier, and by Agassiz, and is known as the Dilatation theory.

Agassiz, its best known advocate, states his case thus : " Ce mouvement paraît plutôt être dû à la dilatation de la glace résultant de la congélation de l'eau qui s'infiltre continuellement dans les fissures capillaires que présente la glace, dans toute son épaisseur, et surtout à la partie la plus voisine de la surface où elle est moins compacte. Cette eau, dont la température est constamment voisine du point de congélation, se transforme en glace au moindre abaissement de température, et tend à dilater le glacier dans tous les sens. Cependant, comme il est contenu des deux côtés pas les flancs de la vallée, et en amont par le poids des masses supérieures, toutes l'action de la dilatation, aidée d'ailleurs de celle de la gravitation, se porte dans le sens de la pente, vers le seul côté qui offre une libre issue." [9]

[9] Notice sur les glaciers, published with Desor's " Journal d'une course aux glaciers," etc., 1840.

When J. de Charpentier read a paper in 1838 before the Helvetic Society of Natural Science on the dilatation theory, M. Merian replied that if the theory were true, glaciers ought to augment in height alone, since the direction of least resistance would be vertically. In this he was supported by M. Studer, who quoted what takes place when anhydrite is converted into gypsum, or lime into dolomite, when the mass swells upwards. This view was also pressed by Hopkins. But the notion that a glacier swells upwards is quite contrary to the careful observations of Forbes. Studer also pointed out that the nocturnal cold only freezes a very superficial layer of the glacier, and that in order that the water should freeze in the crevasses or cracks the temperature must be below zero of Réaumur, which is lower than we know the temperature to be at the base of glaciers.[1]

Hopkins argued that while it is true that water freely percolates through certain kinds of glacier ice, it cannot be proved that it freezes in the insterstices. " The temperature of the upper portion of a glacier," he says, " where the percolation has been observed, is very little below that of freezing, and does not appear to be sufficiently low to convert water into ice while moving with the freedom with which it descends through the glacier. Wherever congelation does take place, the capillary pores must be filled up, and where it does not, the percolating water must proceed till it meets with the larger fissures, through which it will descend freely to the bottom of the glacier.

Forbes's refutation of the theory of dilatation was twofold. He replied to it on *à priori* and on experimental grounds. Thus he says : " The dilatation theory is founded on a mistake as to a physical fact. . . . The maximum temperature which a glacier can have, observes M. de Charpentier,[2] is 0° centigrade or 32 Fahrenheit, and the water in its fissures is kept liquid only *by the small quantity of heat* which reaches it from the surface water and the surrounding air. Take away this sole cause of heat, i.e. let the surface be frozen, and the water in the ice must congeal. Now this is a pure fallacy ; for the fact of the latent heat of water is entirely overlooked. The latent heat of water expresses the fact that where that

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[1] Mémoires, op. cit., pp. 113-114.   [2] Essai, pp. 9 and 104.

fluid is reduced to 32° it does not immediately solidify, but that the abstraction, not of a small quantity but a very large quantity indeed, is necessary to convert the water at 32° into ice at 32°. Not a great deal less heat must be abstracted than the difference between the heat of boiling water and that at common temperatures. The fallacy, then, consists in this : Admitting all the premises, the ice at 32° (it is allowed that in summer during the period of infiltration it cannot be lower) is traversed by fissures extending to a great depth (for otherwise the dilatation would be only superficial) filled with surface water at 32°. Night approaches, and the surface freezes, and water ceases to be conveyed to the interior. Then, says the theorist, the water already in the crevices and fissures of the ice, and in contact with ice, instantly freezes. Not at all; for where is it to deposit the heat of fluidity, without which it cannot under any circumstances assume the solid form ? The ice surrounding it cannot take it; for being already at 32°, it would melt it. It can, therefore, only be slowly conveyed away through the ice to the surface on the supposition that the cold is sufficiently intense and prolonged to reduce the upper part of the ice considerably below 32°. The progress of cold and congelation in a glacier will therefore be, in general, similar to that in the earth, which, it is well known, can be frozen to the depth of but a few inches in one night, however intense the cold. Such a degree and quantity of freezing as can be attributed to the cold of a summer's night must therefore be absolutely inefficient on the mass of the glacier." [3]

This reasoning seems unanswerable. Forbes elsewhere refers to an experimental proof. He says : "The most direct observation shows that the nocturnal congelation which is so visible at the surface, drying up the streamlets of water, and glazing the ice with a slippery crust, extends, but to the most trifling depth, into the mass of the glacier. This is so evident upon consideration that, when fairly placed before him, M. de Charpentier has been obliged to abandon the idea that the diurnal variation of temperature produces any effect. In truth there is positive evidence that no internal congelation takes place during the summer season, when the motion is

[3] Forbes's Travels, 36-7.

most rapid, and when, therefore, the cause of motion must be most energetic." He then goes on to describe how on one occasion he traversed the Mer de Glace up to the higher part of the Glacier de Lechaud, while it was covered with snow to a depth of six inches at Montanvert, and three times as much in the higher part. It was snowing at the time, and for a week the glacier had been in the same state nearly, the thermometer having fallen, meanwhile, to 20° Fahrenheit. . . . All the superficial rills were frozen over, there were no cascades in the " Moulins," all was as still as it could be in mid-winter; yet even on the Glacier de Lechaud my wooden poles, sunk to a depth of less than a foot in the ice, were quite wet, literally standing in water, and consequently unfrozen to the walls, and in the hollows beneath the stones of the moraines, by breaking the crust of ice, pools of unfrozen water might be found almost on the surface."[4] Forbes goes on to say that "if the dilatation theory were correct, a sudden frost succeeding wet weather must inevitably cause the glacier to advance far more rapidly than in summer, or indeed at any other season, for there could never possibly be more water to be frozen or could cold ever act with more energy than at the time in question, but the contrary was found to be the case, and directly the severe weather passed and the little congelation which had taken place thawed, and the snow was reduced to water, then the glacier, saturated in all its pores, resumed its march nearly as in the height of summer." Thirdly, he urges that the well-established motion of glaciers in winter is directly inconsistent with the views of those who urge dilatation as the result of alternate congelation and thaw, and as the motive force which impels them, since, when the glacier is completely frozen, this cannot occur. The fact that the motion of the glacier during the day and night is sensibly uniform points the same way. If the theory were true, again, the motion of the glacier ought to vanish near its origin and increase continually towards its lower extremity. "I have found," he says, "the motion of the higher part of the Mer de Glace to differ very little from that several leagues further down; while in the middle, owing to the expansion of the glacier in breadth, its march was slower than in either of the other parts."[5]

[4] Id. 358.   [5] Theory of Glaciers, 33-4; Travels, 363-4.

In addition to these arguments, Heim urged that, since water only expands to the extent of one-ninth of its volume in freezing, the total motion which ensues from the freezing of the water in the capillaries of a glacier is very slight, and further, that as the glacier gets consolidated, these capillaries gradually disappear, and with their disappearance disappears also the *primum mobile* postulated by the dilatationists. He also urged that if the dilatation theory were true, the maximum of motion in a glacier should be greatest in the evening, when the water in the capillaries would freeze, or in the morning when it should melt, which is not found to be the case.[6]

Bonney, in regard to this theory, says it fails to explain the motion of a glacier in the coldest winters, and the fact that it is not liable to any marked change when there is a sudden alteration in the temperature of the surrounding air. Nor have its advocates proved the existence of the fine capillaries necessary for it to work upon. On the contrary, Huxley and Tyndall showed that, except at the surface, a glacier is formed of compact ice impenetrable to coloured liquids.

In addition to these various converging and irresistible difficulties, the dilatation theory does not account for the fact that the motion of glaciers has been shown to exist in the deep blue ice, where there are no fissures, as well as in the superficial ice. Nor for the fact of the continuously differential motion which has also been shown to pervade all parts of the glacier. The theory has long been discarded, and was, I believe, discarded by its greatest champion, Agassiz, before he died.

We will now turn to a development of the dilatation theory, which still has some supporters.

As early as 1822 Hugi had noticed the granular structure of glacier ice to which I have referred, and he urged that if we examine the most compact ice of glaciers when it is melting, this granular structure is displayed.[7] He further showed that the size of the glacier crystals increases progressively from the higher part of the glacier to the lower, and that they

[6] Heim, Gletscherkunde, 294-5; see also Mousson's Gletscher der Jetztzeit, 155, etc.
[7] Hugi, Die Gletscher, etc., 8, etc.

are in a state of gradual growth. This observation has been amply confirmed,[8] and notably by Bertin and Grad, who employed the polariscope for the purpose. It must be confessed that the process here described presents some very great puzzles and difficulties for the physicist. How the crystals in a compact mass of ice can grow from the size of lentils to that of a hen's eggs, granting even that the growth takes centuries to develop, is a very great puzzle. They clearly can only grow by in some way attracting fresh water to themselves. Hugi supposed that the water which they enlist comes to them in the form of atmospheric vapour, since his experiments had shown him that the mass of the glacier was not permeable to liquids, a result in which, as we have seen, he was confirmed by Huxley, and he further went on to urge that it is by this growth that the movement of ice in glaciers is in fact produced.

Grad contended that in his own experiments, infiltration of water was shown to be possible,[9] and went on to say that the casue of glacier movement is the infiltration of water into the capillary fissures and its subsequent freezing on the crystals of ice forming the glacier, which are consequently enlarged and made to assume a constant instead of a heterogeneous direction like those of frozen water. The expansion thus caused develops a general movement of the ice in the direction of least resistance; in other words, he says : " la masse même du glacier s'accroît par intersusception, et c'est ce développement ou cette croissance qui provoque le mouvement."[1]

Practically the same views were pressed at greater length and with greater elaborateness by Forel, who, in 1882, published a paper in the *Archives des Sciences physiques et naturelles* of Geneva, entitled " Le Grain du Glacier." He contended that while the glacier crystals are very compact, they are bounded by capillary fissures in which water circulates freely when the glacier *is at melting temperature*, and he explained that Hugi's results were consistent with this view. They were made with ice at lower temperatures. He urged with Grad that the crystals increase from the water which

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[8] Id. 9, etc.   [9] Comptes-Rendus, cxix. 957.
[1] Comptes-Rendus for 1867, vol. lxiv. 46-7.

melts in summer and permeates the glacier, and freezes in winter. He distinguished between his own view and that of Charpentier. The latter, he urged, attributed the movement of the glacier to the dilatation caused by the passage of water into ice in the capillary fissures. According to the alternative theory, the continuous increase in bulk of the glacier is due to the continuous enlargement of the small crystals which compose it, the growth of the various parts combining in a growth and therefore in a movement of the whole.

Hagenbach, in order to account for the growth of the crystals, instead of postulating an infiltration, urged that the crystals absorb each other, and thus increase at each other's expense. In regard to Hagenbach's view, Forel said that, if true, we ought to find in the " glacier grains " a marked inequality of size, some growing and some diminishing in size, whereas the mass of the glacier is formed of grains of virtually the same size in the same district.[2]

Hagenbach to some extent concedes this objection as a valid one, but suggests that the infrequency of the occurrence of small crystals may only mark the surface layers.

In regard to Forel's view, it involves equal, if not greater difficulties. In order that crystals of ice may increase in size in the interior of a glacier, its temperature must fall considerably below zero. Forel himself has calculated that if there is to be an annual increase of 0·043 in volume, or 0·014 in length in a glacier crystal, the temperature of the glacier must sink to −7, a conclusion which is certainly not borne out by the experience we have of the temperature of glaciers. Again, so far as we know, the crystals forming a glacier are in contact with each other when its temperature is below zero. Capillary fissures only develop between the crystals when the ice begins to melt. These fissures do not apparently exist at all, nor is ice permeable to liquids when below the freezing point, and at a temperature therefore where the growth of the crystals can alone take place, and if we limit the actual growth of the glacier crystals to the winter, when the temperature may perhaps be sufficiently low, we cannot appeal to that growth to explain the motion of glaciers, which is greatest in summer.

If we are to attribute the continuous motion of the glacier

[2] Le Grain du Glacier, 334.

to this growth of its crystalline components, we must also remember how very slight the motion would be. The water which permeates the capillaries merely fills spaces already in existence, and cannot, therefore, by itself cause any thrust, while in freezing, its bulk is only increased by one-ninth of itself; nor can we well see how, when the fissures and capillaries are filled by the infiltration of water in the spring, they can re-open again. No force is available for the purpose. It is not enough to appeal to the water and to the effect of freezing in dilating it—we must also find some force by which the separate crystals of ice shall each year be pulled asunder so as to again cause voids.[3]

Heim, in criticizing the theory to which in a measure he was favourably inclined, urged first, on broad grounds, that if, as Forel argued, the growth of the glacier was due to the freezing in winter of the water which permeates it in summer, it is hard to see why in Greenland, where the summers are so short and the winters so long, the ice should nevertheless move much faster than that of the Alpine glaciers.[4] Secondly, the external cold ought first to freeze the water in the superficial capillaries, and then gradually freeze up those in the lower depths of the glacier. If this were so, we ought to find that in the early winter the surface layers move faster than the bottom ones, whereas later on, when the surface water has been some time sealed up and the frost operates deeper and deeper, the motion of the deeper layers ought to be greater than that of the surface ones, which is contrary to experience.

If the glacier motion is due to a general swelling of its bulk, owing to the enlargement of its component grains, how is it that it does not swell upwards in the direction of least resistance, as well as downwards in the direction of the most compact ice? Its behaviour ought assuredly to be that of quicklime, etc., when charged with water, which swells and pushes out in the direction of least resistance.

Again, if the motion of the glacier is due to the growth of its grains, how are we to account for its moving more quickly in the centre than at the sides? The walls of the glacier no doubt act as a drag on the movement of the glacier by means of friction, but that they exercise a crushing

[3] Id. 364.   [4] Heim, 299.

influence on the ice near the sides so as to prevent the creation of fissures or the infiltration of water there more than in the centre, seems an assumption at isssue with the evidence.

As Heim says, if the size of the glacier grains is a function of the amount and rate of the cooling and melting, and also of the time during which the process has continued, the grains near the sides, which move much more slowly than those in the middle, ought to be ten, or twenty, or thirty times larger, which they are not.

Heim again makes an elaborate calculation to show that if the movement of the glacier is merely the sum of the movements caused by the growth of the crystals, then the observed rate of melting of a glacier would not compensate for the growth, but would be very much too small, so that we ought to find glaciers continuously and rapidly growing in length and size. He concludes, in fact, that the swelling of the glacier caused by the growth of the grains, must be very slight if any.

For these reasons, and for the further reason that it seems impossible to correlate the differential motion of glaciers, as observed by Forbes and others, with any process of a mere general swelling of its bulk, it seems to me that we cannot assign to this cause any but a slight influence in the movement of glaciers.

Let us now turn to another theory which involves dilatation and contraction in another form, namely, that of Mr. Moseley. He had noticed that the lead upon the roof of a church at Bristol gradually descended owing to alternate variations of temperature, and arguing from this, he urged that a glacier's motion was best explained by the alternate expansion and contraction of the ice which forms it, due to variations of temperature. This motion should take place in the direction where it is easiest, namely, down the valley; where expansion would be assisted by gravity, while contraction would be resisted by the same force: thus expansion would gain somewhat upon contraction in every alternation of temperature, and the general centre of gravity of the mass would move down somewhat. Mr. Moseley defended this theory in several papers.

In criticizing it, we must in the first place remember that ice cannot expand with heat when above the freezing point, and, if so, as Mr. Croll pertinently says, how are we to account at all for the motion of glaciers in summer on this theory? When the temperature of ice is below the freezing point, the rays which are absorbed will no doubt produce dilatation; but during summer, when the ice is not below the freezing point, no dilatation can take place. All physicists agree that the rays that are then absorbed go to melt the ice and not to expand it.[5]

If Moseley's theory be correct, we cannot understand why the glaciers in the Arctic regions should advance in winter, and we ought to find proofs of retrogression due to winter contraction as well as of summer progress. On this theory, again, why should the advance of a glacier be greater both at the upper and the lower end than half way up; why more rapid in its medial portion than near its edges? Again, to quote an argument of Mr. Blake's, a glacier for purposes of this experiment is like a piece of ordinary ice, and "if one will flatten out under the influence of heat, the other ought to do. But who ever saw a block of ice bulge out under the influence of heat? If anyone has seen such a thing, or has made any experiments upon it, it would be far more to the point than theory, or if these molecular changes could go on even in a large mass of ice without any *vis-à-tergo*, surely some tendency to a definite shape ought to have been observed in icebergs which should, as the mass widens out, grow thinner and thinner."

Forbes pertinently urges that in order to account for the observed rate of the motion of glaciers by this theory, the entire mass of the Mer de Glace of Chamounix must have an average range of temperature of $4\frac{1}{4}$ Réaumur or $9\frac{1}{2}$ Fahrenheit, which is quite contrary to experience. The expansion and contraction of ice by heat and cold can only take place when it is below its freezing point. If it is percolated by water it cannot rise above 32° or expand, and, as we know it is so percolated during the daytime, we cannot believe that during the night the temperature can be lowered throughout to a depth of from 300 to 600 feet of ice through a range of

[5] Phil. Mag. xl. 166.

9½ degrees. As a matter of fact, according to the observations of De Saussure and others, the actual range of temperature attributable to a glacier is between limits absolutely incapable of affecting the expansion of the ice in the smallest degree.[6]

Mr. Matthews, in a paper published in the *Alpine Journal* for 1870, says :—"The whole superstructure of the crawling theory is founded on the hypothesis of the variation in temperature of the interior of glacier ice, and until that hypothesis is verified experimentally the theory cannot be translated from the region of speculation into that of reality. To Mr. Moseley's statement, that ice if opaque to non-luminous is transparent to luminous heat, he urges that this cannot apply to those portions of a glacier above the snow line, nor to those portions of it below that line which belong to the region of *névé* which share in the movement of the general mass. Even in the region of the glacier, when the substance is actual ice itself, it is doubtful if the sun's heat penetrates many inches. The surface bears a great resemblance to the upturned edges of a pack of slates, and it becomes very opaque as it disintegrates with the sun's heat. Nor would the sun's rays reach those portions of a glacier covered with moraine and rubbish. In such parts, therefore, the motion ought to be greatly diminished or to be entirely arrested. If ice dilates with heat like lead, there must be a point above which it does not expand, and where its own motion will be nil. Above this point, if it is below the summit level, the glacier will move uphill or be crushed in its attempt to do so, and below it, each point will move with a velocity proportional to its distance from the point at rest, which is contrary to all experience."[7]

Mr. J. Ball, in the *Philosophical Magazine* for July, 1870, argues that a glacier cannot move *en masse* by expansion and contraction, since it is not a solid mass, but cut up into sections by crevasses much deeper than the depths affected by external changes of temperature. Again, all the experiments (notably those of Agassiz) point to the interior of a glacier having a more or less constant temperature, and not being subject to great variations ; its surface being coated

[6] Id. 41.   [7] Alpine Journal, 1870, 421.

with a nearly opaque crust, protects its interior from any but trifling influences of luminous heat. The winter cold does not penetrate the surface more than a moderate number of feet, and that of the night scarcely as many inches. Again, if Moseley's theory be true, a glacier ought to progress at a rate proportional to its length, which it does not. In winter glaciers are covered with snow, which protects them against the effect of radiant heat, yet they move on as they do in summer, only at half the pace. How, again, by such a theory, can we account for the differential motion between the centre and the sides of a glacier?[8]

After Canon Moseley's death his theory was revived by Mr. Brown, in a paper read before the Royal Society.[9] In this he somewhat modified the original view, in order to meet objections, and urged that the contraction and expansion of the surface layers of a glacier drag the lower layers after them, and cause the upper layers to shear over those below them. To this particular argument it may be urged that the notion that the contraction and expansion caused by diurnal variation of temperature on an Alpine glacier, which directly affects only two or three feet of its surface, can influence its motion for a thickness of several hundred feet, is assuredly extravagant. Again, the recurrence of crevasses must prevent the contraction between portions of the surface layer at any considerable distance from one another.[1] This concurrence of evidence seems to put Mr. Moseley's theory, which a few years ago was advocated with so much pertinacity and success, out of court altogether, nor do I know that it retains any supporters. Let us now turn to another theory, which still has some adherents.

In the year 1845 Mr. Sutcliffe communicated to the *Philosophical Magazine* a paper on a theory of glacier movement. In this paper he proposed to reconcile the apparent contradiction between the observed action of glaciers and the apparently rigid nature of ice by postulating that heat is actually developed in glaciers by the intense local pressure in their interior. This heat, he urges, must create temporary fluidity at points and surfaces where the compressing

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[8] Ball, Phil. Mag., xl. 1—10.   [9] Proceedings, vol. xxx.
[1] Trotter, Proceedings Royal Society, xxxviii. 93.

force is a maximum, thereby allowing the particles to slide into new positions, until when released from the excess of pressure, the mass instantly resumes its rigidity. He argues also that pressure without heat may tend to reduce ice to fluidity. From the fact of water being denser than ice it follows that, if water were cooled down below the freezing point while subjected to pressure, it might be found to remain permanently fluid, whence it would be fair to presume that pressure sufficiently great would restore ice to the more compact form of fluid water. He concludes by suggesting this fact as a possible *vera causa* of the motion of glaciers.[2] His paper, which is very interesting and suggestive, has been largely overlooked. It in fact propounds as an hypothesis a view put forward by Professor James Thomson many years later with considerable force.

That investigator argued in a paper published in 1849 that the lowering of the freezing point of water ought to amount to ·0075° centigrade for every additional atmosphere of pressure. His conclusion was experimentally proved by Sir Wm. Thomson in 1850.

On the basis of this as a postulate, Professor Thomson, in 1857, went on to argue that, " if a mass of slightly porous ice containing water diffused in it at 0° centigrade be subjected to forces tending to change its form, it will have its melting point lowered below 0° centigrade, and will therefore begin to melt and in liquefying evolve cold; the liquefied portions, being subjected to squeezing of the compressed mass in which they originate, will spread themselves out through the pores of the general mass by dispersion from the regions of greatest to those of least fluid pressure. This will relieve the pressure where the compression and liquefaction of the ice takes place; on the removal of the pressure the water will be frozen by the cold liberated, as already mentioned; the water thus freezing in a new position will cause a change of form, and a plastic yielding of the mass of the ice. The yielding of one part leaves another part free from pressure, and that acts in a similar manner, and on the whole a continual succession goes on of pressures being applied to particular parts, liquefaction in those parts—dispersion of the

[2] Phil. Mag. xxvi. 495-7.

water so produced, in such directions as will relieve its pressure and recongelation by the cold previously evolved, of the water on its being relieved from this pressure. The parts recongealed after having been melted must in turn, through the yielding of other parts, receive pressures from the applied forces, thereby to be again liquefied and to enter again on a similar cycle of operation."

Professor Thomson adds a note to the effect that the case is not limited to ice originally porous. If ice be kept at or above 0° centigrade, then as soon as pressure is applied to it, pores occupied by liquid water must immediately be formed in the compressed parts, and no part of the ice, however solid, can resist being permeated by the water squeezed against it, which by its pressure must cause melting to set in, thereby reducing it to a porous condition.[3]

The objections to this theory, countenanced as it has been by some great names, including that of Helmholtz, are insuperable. Thus, as Mr. Brown urges, " its advocates hardly seem to consider how very small the lowering of the freezing point is for any ordinary pressure. It is only ·0075 per atmosphere. In other words, it will require a pressure of 2000 lbs. per square inch to liquefy ice at 31° instead of 32°. This is equivalent to the weight of a column of ice about 5000 feet high. It is needless to ask whether such a pressure can exist within an ordinary glacier, while, on the other hand, glaciers undoubtedly move at temperatures far below the freezing point, in the arctic regions, below zero." [4]

Professor Tyndall has also proposed some acute criticisms of this theory. He first urges that the water in the supposed case, when escaping, would escape upwards as surely as downwards, since the tendency to flow down by its own gravity would be slight compared with the other forces acting on it. The ice above the melting portion would be less dense and more permeable than that below it, and the glacier ought to move uphill instead of down. Again, as Tyndall says : " The difference between the level of the Mer de Glace at Montanvert, and at the summit of its principal tributary, the Col du Géant, is about 4846 feet. An atmosphere of pressure

[3] Phil. Mag., 4th ser., xiv. 549-550.
[4] Proceedings Royal Society, xxxiv. 211-212.

is equivalent to about 40 feet of ice, which, according to Professor Thomson, would lower the freezing point of water by ·0075 of a degree centigrade." This being so, the pressure of the whole column of ice referred to, 4846 feet, would lower it nine-tenths of a degree. *"Supposing then,"* as Tyndall says, *"the unimpeded thrust of the whole glacier, from the Col du Géant downwards,* to be exerted on the bed at the Montanvert, or, in other words, suppose the bed of the glacier to be absolutely smooth and every trace of friction abolished, the utmost the pressure thus obtained could perform would be to lower the melting point of the Montanvert ice by this quantity. Taking into account the actual state of things, the friction of the glacier against its sides and bed, the opposition which the three tributaries encounter in the neck of the valley at Trélaporte, the resistance encountered in the sinuous valley through which it passes; and, finally, bearing in mind the comparatively short length of the glacier, which has to bear the thrust, and oppose the latter by its friction only, I think it will be evident that the ice at Montanvert cannot possibly have its melting point lowered by pressure more than a small fraction of a degree." Tyndall then goes on to urge that his experiments in the winter on the surface ice of the Mer de Glace showed considerable movement at $-5°$ centigrade, when it would require 667 atmospheres of pressure to melt it, equivalent to a column of ice 26,680 feet, or to a height two and a half times that of Mont-Blanc above the Montanvert, whose portentous summit must have been connected with it by a continuous glacier, with its bed absolutely smooth to secure the requisite pressure.[5]

Lastly, it seems to me, Professor Thomson's theory fails to meet the difficulty that the motion of a glacier is differential, that its centre moves faster than its sides, and its surface more than its base; that is, that the motion is greatest where the pressure, and therefore the postulated liquefaction, is least.

Let us now turn to Dr. Croll's theory, which, like all his speculations, has the advantage of being ingenious and of endeavouring frankly to meet the conditions of the problem.

[5] Glaciers of the Alps, 342-345.

In a paper he published in 1870 he took for granted that Mr. Moseley's experiments on the shearing of ice, to which reference will be made presently, were conclusive against the notion that ice is a plastic substance. His words are, "Unless some very serious error could be pointed out in the mathematical part of his investigation, it would be hopeless to overturn his general conclusion as regards the received theory of the cause of the descent of glaciers, by searching for errors in the experimental data on which the conclusion rests." [6]

He, on the other hand, saw clearly that none of the theories already described would account for the differential motion of glaciers as proved by numerous experiments, and that unless this difficulty could be explained, it was useless to produce an explanation, however ingenious. He accordingly appealed to a modification of the theory of Sutcliffe and of Thomson, which I had better state in his own words. "It is found," he says, "that the rate at which a glacier descends depends upon the amount of heat which it is receiving. . . . heat assists gravitation to shear the ice not by direct pressure but by diminishing the cohesive force of the particles, so as to enable gravitation to push the one past the other. . . . There seems to be but one explanation, namely, that the motion of the ice is molecular. The ice descends molecule by molecule. . . . The passage of heat through ice, whether by conduction or radiation, is in all probability a molecular process, that is, the form of energy termed heat is transmitted from molecule to molecule of the ice, a particle takes the energy from its neighbour and passes it on, but a particle must be in a different condition when in possession of the energy to what it is before and after. Before it was in a crystalline state it was ice, and after it will be ice, but at the moment it is in possession of the passing energy it becomes water. We know that the ice of a glacier in the mass cannot become possessed of energy in the form of heat without becoming fluid. May not the same thing hold true of the ice particle?" [7]

He urges, in effect, that the shearing force of the particles of ice when heat is passing through them is not constant, and

[6] Phil. Mag. xl. 154.   [7] Id. xl. 168-9.

"that while a molecule of ice is in the act of transmitting the energy received, it loses for the moment its shearing force if the temperature of the ice be not under 32° F. Consequently a molecule, directly it assumes the fluid state, is completely freed from shearing force, and can descend by virtue of its own weight without any impediment. All that the molecule requires is simply room or space to advance in. If the molecule were in absolute contact with the adjoining molecule below, it would not descend unless it could push that molecule before it, which it probably would not be able to do. But the molecule actually has room in which to advance; for in passing from the solid to the liquid state its volume is diminished by about one-tenth, and it consequently can descend. But when it again assumes the solid form, it will regain its former volume; but the question is, will it go back to its old position? If there were only this one molecule affected by the heat, this molecule would certainly not descend; but all the molecules are similarly affected, although not all at the same moment of time. At the lower end of a glacier a molecule receives heat from the sun, melts, losing its shearing force, descends by its own weight, and contracts. The next molecule above it is then at liberty to descend, and will do so as soon as it assumes the liquid state. The former molecule has meanwhile become solid, and again fixed by shearing force, but it is not fixed in the old position, but a little below where it was before. If the second molecule has not meanwhile melted from heat derived from the sun, the additional supply it will receive from the solidifying of the first one will melt it. It will then immediately descend till it reaches the first molecule, when it in turn becomes solidified, and so the process goes through the glacier, which is consequently in a state of constant motion downwards." [8]

To this exposition of Mr. Croll's theory Mr. J. Burns replied in the *Geological Magazine* for 1876. "It seems strange," he says, "that a molecule A should on freezing give its heat to B, from which it is some distance removed, and should impart none to the molecule on which it rests, or to

[8] Phil. Trans., xxxvii. 201-204.

those on either side, but supposing the molecules to be perfectly accommodating in this respect, their downward progress is only helped by heat passing along the glacier from its lower end upwards. Let us take the molecules A, B, and C, somewhere within or on the surface of the glacier. Supposing B now is melted by the sun. When a solid, it was in contact with both A and C, and therefore on melting it cannot move. True, when liquid it is reduced about one-tenth in size, and in consequence its centre may move a small fraction of its diameter towards A ; but on freezing again it must resume its original position. A or C now gets the heat, melts, oscillates, and freezes in its old position ; and so on. From such heat there is plainly no molecular motion." Even if we were to modify Mr. Croll's theory so as to make the freezing of one particle synchronous with the melting of its neighbour, we should secure only the smallest imaginable molecular motion. In reference to the denuding power of glaciers, which Mr. Croll explains as due directly to the stones and other hard matters embedded in them and propelled along by the molecular movement he appeals to, Mr. Burns replies forcibly that there could not be a weaker denuding agency than a great glacier which as a solid mass is stationary, but on whose interior and on whose surface liquid molecules are here and there moving through infinitesimal distances . . . . will a stone that is held in the crystalline grasp of millions of ice molecules be forced along by a few dozen water molecules trickling through a fraction of their diameter along its surface. A stone thus ' forced along,' may be supposed to scoop out valleys if the exigencies of geologists demand it, but the force that moves the stone would not serve to tickle the sole of a mite. . . . The motion of the molecules within a glacier can no more cause a thrust than the rise of the sap within a tree in spring can pull it up by the roots."

Dr. Croll did not answer this attack, but in a subsequent paper he professes to have abandoned the views he originally propounded on the subject, and to have modified his position considerably. In this paper he says : " Ice is not absolutely solid throughout. It is composed of crystalline particles, which, though in contact with one another, are however not

packed together so as to occupy the least possible space, and even though they were, the particles would not fit so closely together as to exclude interstices. The crystalline particles are however united together at special points determined by their polarity, hence, as Professor Tyndall remarks, the reason why volume for volume ice is less dense than water . . . When a crystalline molecule melts, capillary attraction will cause it to flow into the interstices between the adjoining molecules. The moment it parts with the heat received it will resolidify, but not so as to fill the cavity it occupied in the fluid state. The liquid molecule in solidifying assumes the crystalline form, and as the interstice in which it solidifies will be too narrow to contain it, the result will be that the fluid molecule, in passing into the crystalline form, will press the two adjoining molecules aside in order to make sufficient room for itself between them, and this it will do, no matter what amount of space it may possess in all other directions. The crystal will not form to suit the cavity, the cavity must be made to contain the crystal, and what holds good of one molecule holds true of every molecule which melts and re-solidifies. The process is therefore going on incessantly in every part of the glacier, and in proportion to the heat which the glacier is receiving. This internal molecular pressure, resulting from the solidifying of the fluid molecules in the interstices of the ice, acts on the mass of the ice as an expansion force, tending to cause the ice to widen out in all directions."

This is the theory of the mechanism of ice motion as finally developed by Dr. Croll. It has been sifted with critical skill and acumen by the Reverend J. F. Blake, in the third volume of the *Geological Magazine*, p. 493, and so far as we can see has been completely shattered by him. At all events his analysis has never been met in any way. As Mr. Blake so well showed, in speaking of crystalline molecules and liquid molecules of the same substance as if they were different in size or shape, Dr. Croll used language without meaning in physical science; a molecule is the ultimate factor to which we can reduce any piece of matter, and being so, is absolutely unalterable in shape and in size; we cannot divide it or alter it without constituting a new substance altogether. The

different forms assumed by each substance are not due in any way to an alteration in its ultimate factors, namely, its molecules, but to the re-arrangement of these same molecules. We cannot, as Dr. Croll does, speak of the melting of a crystalline molecule, and of its flowing into the interstices between adjoining molecules, nor can we speak of a liquid molecule solidifying and assuming the crystalline shape. This language, and the whole induction based upon it, ignores entirely the real nature of molecules. If we put aside this fundamental contradiction, and understand Dr. Croll as referring not to molecules but to the minute particles which are aggregated together in all masses of ice, each particle consisting of a congeries of molecules, which when it melts are loosened, we are no nearer a rational solution of our difficulty.

Suppose we grant that, in melting, these particles lose their polarity and arrange themselves so that they get better into the spaces between those forming part of the crystalline substance, and better also among themselves, so that on the whole they occupy less room than before, Mr. Croll seems to argue that in solidifying these particles will find the spaces or interstices in which they solidify too narrow for them, and must consequently squeeze their neighbours asunder and thus cause the ice to expand. To this Mr. Blake replies : " Is the fragment or particle of ice to which the argument is applied divisible or indivisible under the ordinary forces of nature ? If divisible, why does it not flow into *several* interstices, and get squeezed into more in the act of solidifying, and make several small crystals instead of one bigger one ? If indivisible the main mass would remain in the original cavity, and could only crystallize by coming back together, or by the main mass coming after the minute portion that had got entangled in an interstice. Again, why should not in every case the molecules whose attachment is loosened by melting go back to their old place in solidifying ? " None that Mr. Croll mentions. " Unless some new force is brought into play," says Mr. Blake, " on the instant, whatever old forces they overcame originally in the act of crystallizing the first time, they can certainly overcome again. Gravity always acted, and the molecular forces of crystalliza-

tion are just as competent to push back the molecules against
it as they were before when first the ice was formed."

"There is nothing," he continues, "in Mr. Croll's theory
to distinguish a glacier from an ordinary piece of ice, and if
one will flatten out as he supposes, the other ought to do
so too. But whoever saw a block of ice bulge out under
the influence of heat? Again, Mr. Croll, in arguing about a
small portion of ice melting and then resolidifying, and
passing its heat on to the next, and so on, seems to ignore
the elements of latent heat and of conductivity in ice.
Before a particle of ice can melt, it must first be brought to
the melting point, *after which* so many units of heat must
be added to it in order to melt it. Suppose a melted
particle in the midst of ice colder than at melting point, its
heat would be distributed in raising the temperature of the
surrounding ice, according to its conductivity, and what
was left would be insufficient to melt any other particle of
its own size. Hence, before the solidifying of one particle
could be sufficient for melting another, the whole surround-
ing mass must be at melting point, and there must further
be some cause for the devotion of the spare heat to one
particular particle, to say nothing of the cause which is to
bring about a perpetual doing and undoing of the same
operation. If there could be any such passing on of a
melted state through a body to the other end, we ought to
see a glass rod held in the fire melting at the end away
from the fire, or does the outside particle refuse to melt?
A candle, too, ought to melt in the socket instead of near the
lighted wick, and to bulge out into abnormal obesity.
When heat is applied to ice the heated side is first raised to
the melting point, and as we recede from that side the tem-
perature gradually diminishes. If more heat is applied
it is spent in keeping up this state of the ice as to tem-
perature against all possible losses, and in melting that part
of the ice nearest to it. No amount of this additional heat
will alter the state of the interior, except so far as it may
alter the *other* conditions on which it depends. . . . No
amount of internal heat could possibly bring about internal
melting in a uniform mass of ice. It is simply a myth."[1]

[2] Geol. Mag., 1876, 495-7.

Again, as Heim says, Croll neglects the fact that ice is transparent to heat rays to some depth ; but when we have considerable thickness, as on a glacier at 0° of temperature, the heat rays are not transmitted, but go to melt the surface layers. Croll's postulate does not account for the quicker progress of the centre than the sides of a glacier, nor for crevasses, etc., etc.[2]

Again Croll's theory requires that his glacier shall remain at the temperature of 32°, whereas we have every reason to believe that it is much below that temperature during a large part of the year, not only on account of the rarefied atmosphere in which it lives but, the continual radiation from its surface at night and evaporation at all times, and except in its lower layers, when the glacier is constantly passing into a liquid form, it is most improbable that the temperature in any part of a glacier is so high as 32. It cannot be urged that when a minute crystal of ice melts, the liquid thus formed drains away between the interstices of its neighbours, or we should have every slab of ice subject to solar rays sweating away its substance from its nether surface, which is not the case. So far as we can experiment, we find that the sun's rays acting on a mass of ice melt its surface layers, and they flow away by gravity, or remain as tightly enclosed by the nether ice where gravity cannot work as water in a well-puddled reservoir does. But Dr. Croll ignores this every-day melting of ice altogether, and introduces us to a transcendental method.

This completes the roll of the various theories which have been forthcoming to account for the motion of glaciers independently of gravity, and it must be admitted that they all fail to reconcile themselves to the ordinary laws of physics, and fail, therefore, to secure themselves a place among the postulates of empirical science. Let us now turn to the theories which in various ways invoke gravity as the chief motor in glacier motion.

We will begin with the earliest of these theories, namely, the sliding hypothesis.

It was first scientifically stated by G. S. Gruner, in a work published at Berne in 1760, entitled *Beschreibung des Eisge-*

[2] Heim, 308.

birges der Schweizerlandes. In this he refers to the fact of stones on the back of the glacier at Grindelwald having been seen to move gradually down, so that a stone had been noticed to advance 50 paces in six years, and urges that the whole mass of ice which is embowered in the valley moves down *en masse* by its own weight. This movement, he argues, is assisted by the greater humidity of the nether surface of the glacier.

Deluc the elder somewhat modified Gruner's position. He argued that great caverns and hollows are formed underneath glaciers which are thus supported on a kind of ice pillars, when they give way the ice mass gives way, and its tendency is to move down the slope on which it rests. " This," he says, " causes the march down of great masses of ice called glaciers. They do not slide down *en masse,* but piecemeal, the pieces or fields of ice being separated by crevasses which close up as the hinder field overtakes the one in front, and the one in front pushes before it the earth as it advances." Our author writes very modestly, however, and confesses that the subject is full of difficulty.[3]

De Saussure, to whom the sliding theory is generally attributed, no doubt discovered and published it independently, but later, in his work, *Voyages dans les Alpes,* of which the first volume was published in 1779. His statement of it I translate as follows : " Another cause which prevents the excessive growth of the snow and ice is their weight, which presses them more or less rapidly into the valleys where the heat of the summer melts them. The descent of the snow in the form of an avalanche is a known phenomenon to which we shall return. That of the ice, which is more gentle and generally with less noise, has been less observed. Nearly all glaciers, as well of the first as of the second kind, rest on inclined beds, and all those of more than a certain size have beneath them, even in winter, currents of water which flow between the ice and the bed which supports it. It can be understood, therefore, that these frozen masses, following the inclined bed on which they rest, separated by the water from their attachment to the ground and sometimes supported by the water, must slide little by little and descend, following the

[3] Lettres Physiques, etc. The Hague, 1778, vol. i. 140-142.

inclination of the valleys.   It is this gradual but continuous sliding of the ice on its inclined bed which moves it into the low valley."

These sentences contain what De Saussure had to say about the motion of glaciers, and it will be seen that both he and his predecessor, Gruner, contented themselves with the supposition, to use the words of Principal Forbes, "that the mass of the glacier is a rigid body sliding over its trough or bed in the manner of solid bodies."

This view was maintained by De Saussure's followers, notably by Ramond, Kuhn, etc., who all apparently treated glacier motion as a motion *en masse* caused by gravity, due partially to the glacier's own weight, partially to the pressure of the higher ice and *névé*." [4]

Forbes summed up his arguments against this theory thus : " If the glacier slides down its bed, why is not its motion continually accelerated, i.e. why does it not result in an avalanche ? and is it not inconceivable that a vast and irregular mass like a glacier, having a mean slope of only 8° and often less than 5°, can *slide*, according to the common laws of gravity and friction, over a bed of uneven rock, and through a channel so sinuous and irregular that it is often embayed in a valley, whence it can only escape by an aperture of half its actual width ?   On all mechanical principles, we answer, that is impossible.   We may add that many small glaciers are seen to rest upon slopes of from 20° to 30° without taking an accelerated motion ; and this is conformable to the known laws of friction.   It is known, for instance, to architects that hewn stones, finely dressed with plane surfaces, will not slide over one another until the slope exceeds 30°."

He says further, " There is no reason to suppose that either Gruner or De Saussure thought it necessary to take into account the varying form of the channel through which the glacier has to pass, and the consequently invincible barrier presented to the passage of a rigid cake of ice through a strait or narrowed aperture where it occurs." [5]

" It is certain," says Reclus, " that the gradual descent of the glacier in its rocky bed, is not a mere sliding of the mass over its lubricated bed, for it has been several times

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[4] See Studer, Lehrbuch, 1844.   [5] Forbes' Travels, 35.

proved that above the zone in which the mean temperature of the ground is below the freezing point, that is, at about the height of 6600 feet in the Central Alps, the layers of the glacier are frozen to the ground, and cannot detach themselves from it by the mere force of gravity. On their lower surfaces they melt but slowly, except at the spot where the rivulet flows, which gathers all the surface water rushing through the crevasses. There are instances also of a stream running by the side of a glacier from the *névé* to the terminal moraine without being able to penetrate the solid wall of ice adhering to its bed of rock."

Hopkins, who resuscitated De Saussure's theory in another form, in replying to Forbes, denied that the bed of a glacier is rough and rugged. "How," he says, "could the hardest rocks resist for thousands of years the increasing effects of infinitely the most powerful polisher that nature has put in action; the fact of the existence of *roches polies* at the end of a glacier and the continuation of glacier valleys proves incontrovertibly that there is some sliding in glaciers," and he urges that the surfaces of rocks forming the beds of glacier must necessarily be free from asperities.

He further showed, experimentally, that, while it is perfectly true that a smooth hard body will not descend down an equally smooth hard plane at angles considerably greater even than that at which some glaciers are formed, ice nevertheless does so; and he argues that whereas the particles of ice in contact with the plane are capable, so long as they remain a part of the *solid* mass, of exerting a considerable force to prevent sliding, they are incapable of exerting any sensible force when they become detached from the mass by the liquefaction or disintegration of its lower surface, and he contended that the essential condition of glacier motion is that its lower surface is kept continually at or near zero by the conduction of heat from the earth's crust, which is proved by the flow of water from underneath all points of glaciers.

Hopkins also proved, experimentally, that the argument of Charpentier and Forbes as to the motion of a glacier, if it moved *en masse*, being an accelerated motion, is only true of angles greater than that whose tangent determines the co-

efficient of friction between the glacier and its bed ; and he further showed that, for inclinations not exceeding 9° or 10° the motion of a moving mass of ice is approximately proportional to the inclination of the slope on which it rests, and that such velocity is increased by increase of weight, and he concluded that *sliding due to gravitation* is a real and very important element in glacier motion. The discussion was making it more and more clear that the scratching of the bed and sides of the valley by stones enclosed like planes in the mass of the ice, and the polishing of large surfaces and the rounding of inequalities shows that in regard to a portion, at all events, of its work a glacier either acts as a rigid body propelled by gravity, or that its lower layers are dragged along by its upper ones.[6]

The chief substantial objection against the theory that it moves entirely *en masse* is, that the motion of its different parts is not uniform, but, on the contrary, there is a differential motion by which its centre moves faster than its sides, and its surface layers faster than the bottom, and this motion is continuous from day to day, and is not made by fits and starts ; so that, granting that a certain movement *en masse* takes place this can only explain one portion of the problem, the greater portion of it remains unexplained. A similar objection applies to the modifications of the sliding theory propounded by Mr. R. Mallet and M. Martins.

The first of these was published in a paper read before the Geological Society of Dublin in 1838. He affirmed that the *primum mobile* which causes the movements of glaciers is hydrostatic pressure acting between them and the rocky bed on which they rest, and thus at intervals lifting them up and floating them, or, as it were, transferring them upon liquid rollers from a higher to a lower level. He goes on to argue that the bed on which a glacier rests is always warmer than the glacier itself, whence the bottom of the glacier is always melting, thus accounting for the torrents which underlie it. This sub-glacier melting, he urges, goes on irrespective of season or climate. In summer the stream is also fed by the melted snow and ice of the surface, the water from which finds its way below by the many fissures. These waters find a ready vent in summer, and, according to

[6] Phil. Mag. xxxvi. 1–16.

Mr. Mallet, the glacier would not move at all in that season but for certain disturbing causes. But to give his own words : "When winter has covered its whole expanse many feet deep in snow, and when the embouchure of the sub-glacial streams is also gelid, and partially, or sometimes wholly stopped, the waters rising and pent up beneath the bed of the glacier, lift its mass more or less from off the base on which it rests, and with more or less regularity according to the variety and size of the several segments into which it is divided by the crevices, until at length a sufficient change of position is effected to permit the escape of the imprisoned waters, when these, rushing forth, empty the icy caverns which they had filled, and the mass of the glacier, whole or in parts, descends by a certain distance into the valley. The motion cannot hence be uniform, but *per saltum*, which is found to be the general fact, though often difficult of observation."[7] This operation, Mr. Mallet argued, is facilitated by the ice being of less specific gravity than water, and he urges that, when the glacier is raised by hydrostatic pressure, it is so in a direction perpendicular to its bed, but, when by the withdrawal of the water it is again deposited, it will be in a direction perpendicular to the horizon; hence the cause of its motion.[8] By means of this theory Mallet professes to account in an ingenious way for many of the phenomena of glaciers. The theory, I need not say, is no longer held by anyone, although it hardly deserves so severe a criticism as was passed upon it by Charpentier, when he says of it :—" Voila une explication qui a besoin de commentaire, mais non pas de refutation."[1] This sharp phrase is due to the fact that he altogether misunderstood the argument of the ingenious Irish writer. It is answered completely, however, by an appeal to the fact that glaciers have been clearly shown not to move by jerks and jumps, but continuously in winter and in summer, and also that they have a continuously differential motion. While his main theory no longer lives, we must not forget that it was Mallet who, in the paper just cited, first noticed that the transverse crevasses in glaciers have a curved form with their convex side presented downwards, and in the direction of the

[7] Trans. Dub. Geol. Soc. i. 319, 320.   [8] Id. 321.
[1] Essai sur les Glaciers, 38 note.

glacier's motion,[2] and that he urged that the central parts of a glacier must descend much more rapidly than its lateral ones.[3] This most important generalization, which has been amply confirmed since, was the first step in a truly scientific theory of glaciers based upon experimental and not upon *à priori* methods.

M. Martins proposed another modification of De Saussure's theory. He states his view thus: "In summer immense transverse crevasses divide the entire mass of the glacier vertically into so many secondary wedge-shaped masses; consequently its surface is increased by the sum of all the spaces which the crevices leave between them at their upper part. The glacier resting firmly against the mountain cannot be pushed backward; it is, therefore, at its lower part, when nothing arrests it, that it becomes displaced and moves forward. The winter following, these crevices are filled with snow, blown into them by the winds, or falling in the form of avalanches. This snow becomes ice under the alternate influences of melting and freezing during the months of May, June, September, and October. In the succeeding summer months new crevices are formed, the glacier advances, and so on successively. This progression is therefore neither a slipping nor a sinking (both of which it is difficult to admit, since the ice must adhere to the ground), but a successive dismemberment.[4]

Besides the objection that we now know, that the movement of glaciers is continuous and not by jerks, Forbes adds, that it is universally admitted that the glacier proper does not grow by the consolidation of snow in its fissures.[5]

The views of De Saussure were revived and modified, as I have mentioned, by a physicist of the first rank, in the person of Mr. W. Hopkins. In his modification of the theory, he got over the difficulty of the differential motion by a somewhat ingenious argument. In his paper published in the eighth volume of the *Cambridge Transactions*, he pressed the view that the friction of the ice against the sides of the valley produces a dislocation of the glacier into longitudinal stripes, and as a result the central portions slide past those adjacent to

[2] Op. cit. 321.   [3] Id. 328.
[4] Ed. Phil. Journ. xxx. 294.   [5] Theory of Glaciers, 101.

them, and so on for successive strips as we approach the sides, the more rapid retardation near the sides being rendered mechanically possible by the increased number of these longitudinal dislocations. The result in such a case, it was argued by Hopkins, is that the ice advances by *échelons*, or that strips of ice of a certain number of feet, or yards, or fathoms, move either suddenly or by gradual sliding, but at all events so as to mark by an abrupt separation at the longitudinal fissure, that one portion of the ice has slipped past the other by a distinct measurable quantity.

In regard to this notion of a glacier being a congeries of moving masses, Forbes maintains that a rugged channel like that of a glacier being packed with angular solid fragments would speedily be choked, and that further pressure from behind would tend to wedge the fragments more tightly. . . . and if the figure of the channel be irregular, i.e. have expansions and contractions, however smooth its surface, and however small the sliding angle, the choking of a strait or contraction by the piling of the fragments will be as complete as if the lateral friction were excessive. This points to the impossibility of the discharge of a fragmentary solid through a gorge by long strips fractured parallel to its length, and constituting parallelopipedons of a certain breadth; secondly, he urges that actual observation proves that a glacier is not a mass of fragments or parallelopipedons, as some have supposed. Most of the crevasses at a small depth shrink to mere slits, and perhaps disappear altogether, and the area they occupy is small compared with that of the unbroken ice. When viewed as a whole, this is capable of conveying strains as thrusts, and its cohesion is no more destroyed than a parchment sieve is incapable of being stretched because it is covered with fine slits. Again, it is seldom the crevasses intersect even when most numerous, and they do not therefore separate the whole mass into blocks or fragments, and when they do so, it would seem that as they are very shallow, they cause only a surface dislocation or they would fall away in avalanches.

Were, he says, the inequality of the central and lateral movement of the glacier mass to be attributed to longitudinal fissures or discontinuities, by means of which broad strips of

ice slide pass each other, we should have to demonstrate the existence of such fissures, which could not be always close unless either (1) the surfaces were mathematically adapted to slide over one another, or (2) the ice possessed sufficient plasticity to mould the surfaces to one another's asperities, in which case the plasticity would alone be sufficient without the discontinuity, to explain the motion of the ice. These longitudinal fissures, cutting the common transverse fissures perpendicularly, would divide the glacier even where most level into trapezia, and no transverse crevasse could be straight edged, but must be jagged like a saw, or cut *en échelon.* Such a phenomenon never occurs unless where a glacier is moving *torrentially* or with great disturbances, and down a steep. *There* such longitudinal fissures may occasionally be seen, but they form the exception and not the rule. It has been demonstrated by an elaborate proof that the only trace of longitudinal discontinuity in the normal condition of the glacier is to be found in the veined structure, which, being caused by a partial discontinuity at a vast number of points, admits of an insensible deformation of the glacial mass without sudden or complete rents, or slips, or the formation of zigzag crevasses.[6]

In the course of this controversy Mr. Hopkins urged that both the sliding and the viscous theories, that is his own and Forbes's theories, agree in assigning gravity as the primary cause of glacier motion, but in the one case the efficiency of gravity is principally due to the state of disintegration of the lower surface of the glacier, while in the other it is maintained that its efficiency is due to the plasticity of the general mass. He does not deny that ice may be partially plastic, but while admitting this he urges that much the greater portion of the movement is due to sliding. He also, with singular fairness, appeals to further experiment as the real test, and states that the observations required are such as will determine, as far as possible, the relative motions of the upper and lower surfaces of a glacier, and he admits that if experiment proves the motion of the upper part of a glacier to bear a large ratio to that of the lower surface, the claim, of the viscous theory must be at once admitted.[7] The test

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[6] Phil. Trans. 1846, 197.   [7] Phil. Mag. 1845, xxvi. 247-250.

last mentioned was again appealed to by Mr. Hopkins in a later paper, in which he says : " The ultimate test of the sliding and plastic theories must be sought in observations on the relative motions of the upper and lower surfaces of a glacier. The claims of the two theories would thus be decided beyond dispute. Accurate observations are also required to ascertain the form which a *continuous* straight line drawn on the surface of a glacier and perpendicular to its axis will assume by the more rapid motion of its central portion. Will it be deformed into a *continuous* loop or into a *discontinuous* one ? Such observations would decide the degree in which the greater central motion is due to the flexibility or plasticity of glacial ice, and the degree in which it is attributable to the dislocation of the general mass. Observations of both kinds are become essential in the present state of glacial theories, and would do much more towards settling the question at once respecting the cause of glacial motion than any further controversy." [8] The appeal was not declined by Forbes.

In 1845 he made some detailed experiments upon the respective motion of glaciers at their surface and base. He made elaborate measurements on the terminal face of the Glacier des Bois at points 8, 54, and 143 feet respectively, above the bed or floor of the glacier. The result confirmed his anticipations, that the effect of friction in retarding motion is most sensible the nearer one gets to the base. The measured motion of the three points was as follows :—

	Feet.	Feet.	Feet.
	2·87	4·18	4·66
being after the ratio of	1·00	2·46	and 1·62.

These results were confirmed by measurements made by MM. Dollfuss and Martins, and published in the *Comptes-Rendus* for October 26th, 1846. Their measurements were made on the lateral face of the glacier, and the two sets of measurements complied exactly with the demand made by Hopkins in the paper already cited. Forbes also showed by careful measurements with the theodolite that the motion of a glacier is perfectly regular and continuous from point to point, and

[8] Phil. Mag. xxvi. 599.

leaves no room for jerks and jumps, such as Hopkins had postulated.

Forbes's experiments may be supplemented by an easy appeal to another kind of evidence. As Dr. M. Williams says: " Crevasses of considerable magnitude are commonly formed without severing one part of a glacier from another. They are usually V-shaped in vertical section, and in many the rupture does not reach the bottom of the glacier. Very rarely indeed does a crevasse cross the whole breadth of a glacier in such a manner as to completely separate, even temporarily, the lower from the upper part.[9] This shows that the upper part of the mass of ice has a greater tension, and moves faster where the tension is removed than the lower. In fact, a glacier is literally a frozen river, and just as the water near the banks of a river is dragged and stopped by friction, its chief motion being in its surface layers, so is a glacier. No doubt the subjacent streams tend to lubricate the ice mass in a measure, doing it more in the day than at night, in summer than in winter, there being then more water available from the melting of its surface. They would do so more if they occupied the whole of its foundation instead of only a part, but this is largely counter-balanced by the tremendous weight of the mass.

It is time that we should now turn to Forbes's own view, namely, the theory that ice, notwithstanding its apparent rigidity, is really of a plastic nature, and that a large part of its motion is due to this quality.

The first person who suggested this was apparently Bordier, who, in 1773, published a work, entitled, *Voyage pittoresque aux glaciers de la Savoie,* in which he compared ice to soft wax, flexible and ductile to a certain point, and attributed to it sufficient ductility to enable it to move down from high ground to low.[1] He was followed many years after by Captain Basil Hall, who, in his work called " Patchwork," describes the glaciers of Miage. He argues that when the successive layers of snow, often several hundred feet thick, are half melted by the sun, and by the innumerable torrents which are poured upon them from every side, to say nothing

[9] Quarterly Journal Sc. vii. 221.
[1] See Tyndall, Glaciers, 133, 134.

of the heavy rains of summer, they form a mass, not liquid, indeed, but such as has a tendency to move down the highly inclined faces on which it lies.[2] Later, in the same work, he compares a glacier with a lava stream, and says they are more or less frozen rivers; they both obey the law of gravitation with great reluctance, being eventually so sluggish that, although they both move along the bottoms of valleys with a force well nigh irresistible, their motion is sometimes scarcely perceptible.[3]

Canon Rendu in his *Théorie des glaciers de la Savoie,* published at Chambery in 1840, was the next to adopt this theory. "The mass of the glacier," he says, "is in inverse proportion to the slope over which it flows. When the trough is steep, the ice is thin, and its surface is contracted; when the slope decreases and approaches to the horizontal line, the glacier fills out—it becomes like a sea, or a lake between two streams. . . . Nothing shows better to what an extent the glacier adapts itself to the spot on which it happens to be than the form of the glacier of Mont Dolent, in the valley of Feriet." The highest plateau is a great amphitheatre surrounded by lofty flakes of granite of pyramidal form; thence the glacier descends by a gorge, into which it is compressed; but as soon as it has passed beyond it, it widens out anew and opens like a fan: it has therefore, as a whole, the form of a sheaf contracted in the middle and spread out at the two extremities.[4] "There are a host of facts that would seem to induce the belief that the substance of glaciers enjoys a kind of ductility which allows it to mould itself upon the locality which it occupies—to thin out, to swell, to contract, and to spread as a soft paste would do. Nevertheless, when we deal with a piece of ice, when we strike it, we find in it a rigidity which is in direct opposition to the appearances of which we have just spoken. Perhaps experiments made upon larger masses would give other results."[5]

Again, the same author writes, "The fact of motion exists, the progression of glaciers is demonstrated; but the mode of motion is entirely unknown. Perhaps with long

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[2] Op. cit. i. 104.
[3] Id. iii. 118.   [4] Voyage dans les Alpes, ii. 247.
[5] Rendu, trans. by A. Wills, edited by J. Forbes, 70, 71.

observations, with experiments upon ice and snow carefully
made, we shall succeed in grasping it; but we are still in
want of first elements. . . . Nothing seems to me more
clearly demonstrated than the progressive motion of glaciers
towards the bottom of the valleys, and nothing at the same
time seems to me more difficult to conceive than the manner
in which this movement is executed—a movement so slow,
so unequal, carried out on slopes of different inclination, on
ground studded with irregularities, and in channels whose
width varies at every moment. This is in my opinion the
least explicable of the phenomena of glaciers. Does it advance
in a mass like a block of marble on an inclined plane? Does
it advance in broken bits like the stones which come down
one after another in mountain gullies? *Does it sink down upon
itself to flow along the slopes, as lava would do, at once ductile
and liquid?* Do the portions which detach themselves at
the edges of steep slopes suffice to impress motion upon those
which repose upon a horizontal surface? I know not. Perhaps,
again, we might say that in times of great cold the water
which fills the numerous transverse crevasses of the glacier,
becoming frozen, receives its accustomed movement of volume,
gives a push to its containing walls, and thus produces a
motion towards the bottom of the channel in which it flows." [6]
Lastly, he says: "There is between the Glacier des Bois and
a river a resemblance so complete that it is impossible to find
in the glacier a circumstance which does not exist in the
river. In currents of water the velocity is not uniform
throughout their width, nor throughout their depth; the
friction of the bottom, that of the sides, the action of obstacles
cause a variation in the velocity, which is undiminished only
towards the middle of the surface. Now the mere inspection
of the glacier is sufficient to prove that the velocity of the
centre is greater than that of the sides. The whole surface is
cut by crevasses, which are in general transverse to its
direction. If the motion were the same throughout the mass
these crevasses which cut the surface in parallel rifts would
form straight lines which would be always nearly perpendicular
to the two banks, but this not so: *the general line is a curve
whose convexity advances towards the bottom of the valley, a fact*

[6] Id. 80-82.

which can only be attributed to the greater velocity of the ice at this point." [7]

These passages, which I feel bound to quote, in consequence of the fierce polemics which have arisen about them, prove that the learned Canon, who afterwards became Bishop of Annecy, fairly grasped the main feature of glacier motion, which had been hitherto neglected, namely, its differential motion. With the modesty of a real student, he does not claim to have proved his case experimentally, but appeals to future observers, who should do so to settle the question. It was not long before the necessary experiments were made.

In the summer of 1841 Principal Forbes was invited by M. Agassiz, who was then studying the Aar glacier, to pay him a visit there. In the course of this visit he realized the necessity of applying precise measurements at different points to the movement of glaciers, so as to definitely settle what the nature of the movement is. The next year he paid a second visit to the Alps, and having made his way to the Mer de Glace at Chamounix, and having pierced a hole in the ice, and planted his theodolite in it, he proceeded to determine its position with respect to three fixed co-ordinates. These having been obtained, three marks were made on rocks, thus giving the absolute position of the point experimented upon. On returning the next day he found that the red mark first made showed that the glacier had advanced 16·5 inches during the previous twenty-six hours. Thus the diurnal motion of a glacier was obtained for the first time from direct observation. [8] During the next four days Forbes satisfied himself by similar methods, (1) that glacier motion is approximately regular and continuous; (2) that it is nearly as great during the night as during the day; (3) that an increase of motion observed on the 20th, 29th, and 30th was due to the heat of the weather; and (4) and most important, that the centre of glaciers moves quicker than the sides, quite contrary to what had been supposed previously. These conclusions he communicated to Professor Jamieson in his "First Letter on Glaciers," dated July the 4th, 1842, [9] and they were amply confirmed in later

[7] Id. 85, 86.
[8] Travels through the Alps of Savoy, 129.
[9] See Ed. New Phil. Journ. xxxii. p. 338-341.

experiments,[1] from which it more clearly appeared that the motion was not the same from day to day and week to week. He also showed that this variation is common to all parts of the glacier, whether compact or fissured, and that the disproportion in the movement of the centre and sides of a glacier is greatest in the lower and faster moving part of it, where it varies from one-third to one-half of the smaller velocity, and least near the origin of the glacier, where it is only one-quarter or one-fifth. The similar variation also affects the centre more than the sides. The greatest daily motion he measured was 27·1 inches.

The veined structure in ice was apparently first described by M. Guyot, in 1838, on the Glacier of Gries. He noticed below, under his feet, he says, furrows an inch or an inch and a half wide, separated by ridges of harder and more transparent ice; the ice consisted clearly of two kinds, one white and melting early, the other more perfect, crystalline, and hard. Their unequal swelling caused the furrows. On looking down a crevasse, which cut the furrows at right angles and showed a transverse section of 30 to 40 feet, the ice seemed everywhere composed of layers of white opaque and transparent ice, as regularly stratified as certain calcareous rocks.[2] Forbes described this structure, which he independently discovered, as the ribboned structure of ice.

The general course of the bands, as pictured on the surface of the glacier, is a succession of oval waves passing into hyperbolas with the greater axis directed along the glacier. The actual shape of the curves depends very much on the configuration of the glacier. In narrow canal-shaped glaciers, the lines are nearly parallel and vertical, inclining upwards and outwards where the ice is supported by the lateral rocks. When the glacier acquires a rounded or oval contour the lines become more or less oval in curve, and dip inwards at angles more nearly perpendicular as the centre of the glacier is approached, and may be compared to sections of inverted cones, having a common apex pointed downwards with its angles continually diminished towards the centre.[3]

[1] Id. 341-345.
[2] See Huber, Les Glaciers, p. 107, quoted by Moseley, Phil. Mag., 4th Ser., xxxix. 241.
Travels in the Alps of Savoy, 29, 160, 372, etc.

The course of the bands being vertical they crop out at the surface, and wherever that surface is intersected and smoothed by superficial watercourses, their structure appears "with the beauty and sharpness of a delicately-lined chalcedony." This structure Forbes proved pervades the whole body of the glacier, and wherever a vertical section is eroded by the action of water, the harder seams of ice stand protuberant, while the immediate ones, partaking of a whitish green in colour, are washed out. He subsequently found that the blue bands are due to compact ice, and the intermediate ones to the ice being frothy and full of bubbles. The structure is apparent throughout the length of the glacier, but is more developed in the neighbourhood of its moraines and walls. That it is not the product of a single season, Forbes showed by tracing the bands across the gaping glaciers. Throughout the greater part of it the bands are parallel to the enclosing walls, but nearer the lower extremity they change their direction and become transverse, and lean forward in the direction in which the glacier moves at a very considerable angle. Forbes argued that "the veined or ribboned structure of the ice is the result of internal forces, by which one portion of it is dragged past another in a manner so gradual as not necessarily to produce large fissures, and the consequent sliding of one detached portion on another, but rather the effect of *a general bruise* over a considerable space of the yielding body. According to this view, the delicate veins seen in the glacier, often less than a quarter of an inch wide, have their course *parallel* to the direction of the sliding effort of one portion of the ice over another." He goes on to quote the case of the Glacier of La Brenva, which at a point where the ice is forcibly pressed against the naked rocky face of an opposing hill is turned into a new direction, and in thus shoving and squeezing past a prominence of rock the ice develops a veined structure so beautiful that it is impossible to resist the wish to carry off slabs, and to perpetuate it by hand specimens. This perfectly-developed structure was visibly opposite the promontory which held the glacier in check, and past which it struggled, leaving a portion of its ice completely embayed in a recess of the shore behind it. Starting from this point as an origin,

the veined laminæ extended backwards and upwards into the glacier, but did not spread literally into the embayed ice. They could, however, be traced from the shore to some distance from the promontory into the icy mass. The direction of laminæ exactly coincided with that in which the ice *must* have moved if it was shoved past the promontory at all. Having proved experimentally that it had so moved, he continues, "No rigid solid body can advance in such a manner, it is therefore plastic, and the veined structure is unquestionably the result of the struggle between the rigidity of the ice and the *quasi-fluid* character of the motion impressed upon it. That it is so, is evident not only from the direction of the laminæ, but from their becoming more distinct exactly in proportion to their nearness to the point from where the bruise is necessarily strongest."

He urges (1) that it accords with the view of the origin of the bands, that the glacier actually does move fastest in the centre, and that the loop of the curves described coincides by observation with the line of swiftest motion. (2) That the bands are least distinct near the centre, for there the difference of velocity of two adjacent strips parallel to the length of the glacier is nearly nothing, but near the sides where the retardation is greatest, it is a maximum. (3) The less elongated form of the loops in the upper part of the glacier corresponds with the observed fact, that the difference in velocity between the centre and sides is greatest near the lower end of the glacier, and that the velocity is most uniform in the upper part. (4) In the highest parts of such glaciers as the curves become less bent, the structure also vanishes (5) In wide glaciers, where the velocity is nearly uniform across their breadth, no vertical structure is developed, while the friction of the base develops an apparent stratification parallel to the slope down which they fall. (6) It also follows that the frontal dip of the structural planes of all glaciers diminishes towards their lower extremity. (7) When two glaciers meet, the structure immediately becomes more developed ; this is due to the increased velocity as well as friction of each, due to lateral compression. (8) The veined structure invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is

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divided into pyramidal masses. This of course destroys any determinate inequality of motion, each mass moving singly.[4]

In regard to the state of the crevasses formed by the laminæ, Forbes showed them to be owing to the differential motion of the parts retarded by lateral friction, and to the fact that the friction being least where the motion is fastest, there will be a natural tendency to molecular dislocation in a direction sloping towards the middle of the glacier. As we near the centre of the glacier, the friction due to the bed of the glacier will more and more modify the effects of that due to its walls, until the lamination will take place entirely in the vertical plain, causing the spoon-shaped arrangement of the surfaces of dislocation as observed. He then goes on to show how the phenomenon of the frontal dip is explainable by the same notion that the glacier really moves as viscous bodies move.[5]

In his famous fourth letter, published in the *Edin. New Phil. Journ.*, January, 1848, Forbes urges most emphatically that ice moves like a plastic body, and after quoting a number of facts in regard to the change in crevasses, inequalities, &c., he says, "All these facts, attested by long and invariable experience, prove that the ice of glaciers is insensibly and continually moulding itself under the influence of external influences, of which the principal, be it remarked, is its own weight affecting its figure, in connection with the surfaces over which it passes, and between which it struggles onwards. It is in this respect absolutely comparable to the water of a river which has here its deep pools, here its constant eddy, continually changing in substance, yet ever the same in form."[6] "The centre of the glacier stream," he again says, "is urged onward by pressure from above, which is there resisted less than at the sides and bottom, owing to the comparative absence of friction. The lateral parts are dragged onward by the motion of the centre, and move also, but it is quite compatible with this idea of semi-fluid motion that the bottom of the glacier should remain frozen to its bed, as some writers have supposed to be the case, though I am far from asserting this to be the fact, or even supposing it probable.

[4] Edin. New Phil. Journ., October, 1842.
[5] Article, "Glacier," in E. B. for 1855.   [6] Id. 34. 4.

. . . . The motion of a glacier resembles that of a viscid fluid, not being uniform in all parts of the transverse section, but the motion of the parts in contact with the walls being determined, mainly by the motion of the centre." And he concludes this famous letter by urging that the admission of some fluid motion in a glacier seems to explain the chief facts of glacier movements :—(1) That it is more rapid at the centre than at the sides, and (2) for the most part, most rapid near the lower extremity of the glacier, but varying rather with the transverse section than the length. (3) That it is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain, and less rapid in sudden frosts. (4) It is farther in conformity with what we know of the plasticity of semi-fluids generally, especially near their point of fusion . . . . such as sealing-wax, for example, exposed for a long time to a temperature far below their melting heat, and which have moulded themselves to the form of the surfaces on which they rest. (5) When the ice is very highly fissured, it yields sensibly to the pressure of the hand, having a slight determinate play, like some kinds of limestone, well known for this quality of flexibility. (6) Such a condition of semi-rigidity accounts for the remarkable veined structure already referred to.

Meanwhile the observations of Forbes, and especially his claims to have discovered the veined structure of ice, were contested by Professor Agassiz, in a memoir published in the same magazine ; [7] and thus began a scientific feud which was not only deplorable in itself, but eventually led to a very serious injury to science itself. I would here remark that in this paper Agassiz reiterates his adhesion to the dilatation theory, and to the conclusion that glaciers do not move in winter, but only in summer.

Forbes continued to press his views as to the viscosity of glacier ice, and notably in his well-known "Travels," published in 1843. In this work he writes, "*A glacier is an imperfect fluid, or a viscous body, which is urged down slopes on a certain inclination by the mutual pressure of its parts.*" He compares glacier ice to a moderately thick mortar or the contents of a tar barrel poured into a sloping channel. " Either

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[7] xxxiii. 265, etc.

of these substances," he says, "without actually assuming a level surface will *tend* to do so. They will descend with different degrees of velocity, depending on *the pressure* to which they are respectively subjected ; the friction occasioned by the nature of the channel or surface over which they move, and *the viscosity*, or mutual adhesiveness, of the particles of the semi-fluid, which prevents each from taking its own course, but subjects all to a mutual restraint. ... . The quantity of viscidity or imperfect mobility in the particles of fluids may have every conceivable variation ; the extremes, are perfect fluidity on the one hand, and perfect rigidity on the other. A good example is seen in the process of consolidation of common plaster of Paris, which, from a consistency not thicker than that of milk, gradually assumes the solid state, through every possible intermediate graduation. Even water is not completely mobile ; it does not run through capillary tubes, and a certain inclination or fall is necessary to make it flow. Water will run freely on a slope of six inches in a mile, or a fall of 1-10,000th part ; another fluid might require a fall of 1 in 1000 ; whilst many bodies may be heaped up to an angle of several degrees before their parts begin to slide over one another.

"Thus a substance apparently solid may, under great pressure, begin to yield ; yet that yielding or sliding of the parts over one another may be quite imperceptible upon the small scale, or under any but enormous pressure. A column of the body itself is the source of the pressure of which we have now to speak.

" Even if the ice of glaciers were admitted to be of a nature perfectly inflexible so far as we can make any attempt to bend it by artificial force, it would not at all follow that such ice is rigid when it is acted on by a column of its own material several hundred feet in height. Pure fluid pressure, or what is commonly called hydrostatical pressure, depends not at all for its energy upon the *slope* of the fluid, but merely upon the *difference of level* of the two connected parts or ends of the mass under consideration. If the body be only semi-fluid, this will no longer be the case ; at least the pressure communicated from one portion (say of a sloping canal) to the other, will not be *the whole* pressure of a vertical column of the material,

equal in height to the difference of level of the parts of the fluid considered ; the consistency or mutual supports of the part opposes a certain resistance to the pressure, and prevents its indefinite transmission. It must be recollected that in the case of glaciers, the pressing columns are enormous, the origin and termination of many of the largest having not less than 4000 feet of difference of level; were they, therefore, perfectly fluid, or suddenly converted into water, the lower end would begin to move with the enormous velocity of 506 feet a second, or would move over *forty-four millions of feet* in twenty-four hours. Now the velocity of the Mer de Glace is only about two feet in that time, a difference so enormous that the fluidity of a glacier compared to water will not appear so preposterous as it might at first sight do, considering the small degree of transmitted pressure required to be effectual.

" Again, it has been attempted to be shown that a glacier is not coherent ice, but is a granular compound of ice and water, possessing under certain circumstances, especially when much saturated with moisture, a rude flexibility sensible even to the hand. Farther, it has been shown that the glacier *does* fall together and choke its own crevasses with its plastic substance."

Forbes then proceeds to argue—

(1) From the proved result that the centre flows faster than the sides and bottom there follows a suggestive corollary. " I have no doubt," he says, " that glaciers slide over their beds, as well as that the particles of ice rub over one another, and change their mutual positions ; but, I maintain that the former motion is caused by the latter, and that the motion impressed by gravity upon the superficial and central parts of a glacier (especially near its lower end), pull the lateral and inferior parts along with them. One proof, if I mistake not, of such an action is, that a deep current of water will flow under a smaller declivity than a shallow one of the same fluid." And this consideration derives no slight confirmation in its application to glaciers, from a circumstance mentioned by M. Elie de Beaumont, which is so true that one wonders it has not been insisted on, namely, that a glacier, where it descends into a valley, is like a body pulled asunder or stretched, and not like a body forced on by superior pressure alone.

(2) The comparatively slight difference between the motion of the centre and sides of a glacier is in accordance with the law prevailing with viscous bodies, that the retardation due to friction will be more completely distributed over the whole section in proportion as the matter is less yielding.

(3) The greatest variation of velocity in a glacier takes place, as it should in a viscous body, near the sides and bottom, while the higher and more central parts move most nearly together.

(4) Forbes confirmed, experimentally, in glaciers, Dubuat's law in regard to the flow of streams, namely, that their velocity at the top and bottom depends upon the actual velocity of the stream, and the amount of lateral retardation depends also upon the actual velocity of the stream.

(5) A glacier, like a stream, has its still pools and its rapids. When it is embayed by rocks it accumulates—*its declivity diminishes and its velocity at the same time;* when it passes down a steep, or issues by a narrow outlet, its velocity increases. The central velocities of the lower, middle, and higher regions of the Mer de Glace are

<div align="center">1·398   ·574   ·925</div>

and if we divide the length of the glacier into three parts we shall find something like these numbers for its declivity,

<div align="center">15° $4\frac{1}{2}$°   8°</div>

Lastly, when the semi-fluid ice inclines to solidity, during a frost, its motion is checked; if its fluidity is increased by a thaw, the motion is instantly accelerated. Its motion is greater in summer than in winter, because the fluidity is more complete at the former than at the latter time. The motion does not cease in winter, because the winter's cold penetrates the ice as it does the ground, only to a limited extent. It is greater in hot weather than in cold, because the sun's heat affords water to saturate the crevices; but the proportion of velocity does not follow the proportion of heat, because any cause, such as the melting of a coating of snow by a sudden thaw, as in the end of September, 1842, produces the same effect as great heat would do. Also, whatever cause accelerates the movement of the centre of the ice increases the difference of central and lateral motion.

Meanwhile, Agassiz continued his researches, and in Desor's

elaborate report of his experiments on the Aar Glacier, we find a reiteration of most of his views, and an elaborate defence of the infiltration theory, with many experiments cited to prove it. On one point he had to give way, however, and to confess that the experiment made by driving a series of six stakes in a line across the glacier as a test of its motion showed that the advance of the glacier caused them to arrange themselves in a curve whose convexity was inclined downwards. This, as Desor reports, far from confirming the opinion which M. Agassiz had previously hazarded, that the margins advance more rapidly than the middle, showed that the centre advances more rapidly than the sides, almost to even double the extent, being in the ratio of 245 feet to 125 in one case and of 269 to 160 in the other.[8]

He, however, still maintained, apparently, that the upper part of a glacier moves more slowly than the lower, and that in winter a glacier is virtually stationary.[9]

In the autumn of 1843, Forbes was again in Switzerland, and, with the assistance of his guide, Auguste Balmat, satisfied himself, by marking rocks, etc., that glaciers move with considerable velocity even in winter.[1] A few months later, on visiting the South of Italy, he was able to compare the motion of a glacier with that of a lava stream. He pointed out that in two respects the comparison fails : (1) In respect of the great liquidity of the lava near its source ; (2) From its unequal rate of consolidation a crust is soon formed more massive than the subjacent mass, and the fluidity is thus not distributed through the mass, " but a tolerably perfect fluid struggles with the increasing load of its ponderous crust, which it tears and rends by the mighty energy of hydrostatic pressure," and thus the crust gets broken, and the whole becomes more like a torrent loaded with blocks of ice than the regulated progress of a glacier, with a graduated retardation towards the sides. In other respects the analogy is more perfect, as he shows in considerable detail. In all these comparisons we must remember that the analogy is not quite complete, or the results would probably be identical ; but, as Forbes remarks, " ice

[8] Edin. Phil. Journ. xxxvi. 155, Bibliothèque Universelle de Geneva, 1843, Nos. 88 and 89.
[9] Id.   [1] E. P. J. xxxvi. 217, 218.

passes from a brittle solid into limpid fluid by heat, while lava passes like sealing wax through every intermediate degree of viscidity."

In the experiments he made in 1844 on the Mer de Glace, which were conducted with great care and minutely measured, he showed by the convexity and regularity of the curves made by the moving points that the movement of the ice is molecular, and, as he says, " proving a regular plastic action of gravity or other propelling force, acting from point to point on the mass of the glacier." [2]

In July, 1844, Forbes applied his methods of observation, which had proved to him the molecular movement of the greater glaciers, to the smaller ones reposing in the cavities of high mountains or on the cols, which De Saussure called glaciers of the second order, and he especially experimented on a small glacier perched on a kind of niche in the northern face of the Schonhorn, about an hour's steep climb above the hospice of the Simplon. These experiments proved that the conclusions as to plasticity drawn from the larger glaciers were amply borne out by the smaller ones, the only difference being that the amount of movement is correspondingly smaller.[3]

The first person to scientifically estimate the rate of motion of glaciers was Hugi, on the Aar Glacier, between 1827 and 1836. He showed that where the measurements were made, the rate of movement was about 2110 feet per annum. Forbes's own measurements were eventually supplemented by those of his Swiss assistant, Balmat, who, in 1844 and 1845, finally put to rest the question as to the movement taking place both in winter and summer. Four posts were inserted in four different positions, with the following result :—

	1 Feet.	2 Feet.	3 Feet.	4 Feet.
Motion for 365 days, Nov., 1844, to Nov., 1845.	847·5	220·8	657·8	489·1
	Inches.	Inches.	Inches.	Inches.
Mean daily motion	27·8	7·3	21·6	16·1
Mean daily motion, summer period, April to October...	37·7	9·9	28·0	22·2
Mean daily motion, winter period, October to April	19·1	4·7	15·8	10·7
Ratio summer to winter motion ...	2·0–1	2·1–7	1·8–1	2·1–1

Phil. Trans. 1845.　　　　　　　[3] Id. 1846.

Forbes says justly that these figures, compared with the records of temperature as observed, confirmed his conclusions of 1842, that the movement of the ice is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain and less rapid in sudden frosts; and he further urges that the velocity of a glacier is largely dependent upon the completeness of its infiltration with water, rendering the whole an imbibed mass, like a sponge, and consequently depends not only on the temperature of any period, but upon the wetness of the surface, whether derived from mild rain, thawing snow, or any meteorological accident.[4]

He thus sums up the various facts presented by crevasses in support of his plastic theory : " The general convexity of the crevasses upwards, notwithstanding the excess of motion in the centre ; the general verticality of the crevasses, notwithstanding the retardation of the bottom ; the perfect state of the crevasses every spring succeeding their visible collapse in autumn ; the ascertained velocity of different parts of the glacier, and the diversity of the annual changes which their velocities present ; the seemingly opposed facts showing the glacier to be subjected to powerful tension, producing crevasses, and yet to be under a compression which produces in some places the *frontal dip ;* and finally, the renewal of the level of the ice during winter, which has been lost partly by superficial melting, but as much or more so by the attenuation and collapse of the glacier during summer." [5]

The experiments of M. Agassiz's staff on the glacier of the Aar continued, and it was with some natural exultation that Forbes declared, as these experiments became more precise, so did their results accord more and more with his. *Inter alia,* they established, he says, that " the movement of the centre of that glacier was to that of a point five metres from the edge as 14 to 1. Such is the effect of plasticity. Thirteen-fourteenths of the motion of the glacier of the Aar are due to the sliding of the ice over its own surface, and one-fourteenth only to its motion over the soil." [6] Shortly after the publication of these words M. Agassiz apparently aban-

[4] Id. [5] Id. 1846.
[6] Ed. Phil. Journ. xxxviii. 339.

doned the dilatation theory, of which he had been so long the champion. We find this change of view reported in the *Bibliothèque Universelle de Genève,* for 1845, p. 347, as follows :—M. Agassiz considère le glacier comme formé d'un assemblage de fragments angulaires de glace, entre lesquels circule de l'eau dans laquelle on voit nager les animalcules vivants. Si l'on jette sur le glacier des liquides coloriés on les voit apparaitre à de grandes distance au fond des crevasses, mais ils ne peuvent pénétrer dans l'intérieur des fragments de glace. *La quantité d'eau qui gorge le glacier parait être la cause de son mouvement, en raison de la pression hydrostatique qu'elle exerce sur la masse.* En effet ce mouvement devient plus rapide lorsque l'eau abonde, et il se ralentit lorsqu'elle vient à diminuer par une cause quelconque ; par exemple une chûte de neige pendant trois a quatre jours de gelée, ce qui oppose à ce que l'eau arrive à la surface du glacier : pendant ce temps il se vide d'eau comme une eponge pressée."

As Forbes says, this passage clearly shows that Agassiz had abandoned the dilatation theory, and accepted that according to which a glacier is a compound of ice and water moving under the impulsion of its own hydrostatic pressure, a view which Forbes himself had constantly urged as explaining the cause of viscosity in glacier ice. As he says: " The hydrostatic pressure within the veins and crevices of the glacier itself can only produce motion by a plastic change in the figure of the mass, and the ductility of the glacier on the great scale becomes a corollary from the admission of internal pressure as a cause of motion." [7]

Forbes, meanwhile, continued to press fresh experiments into his favour. M. Person is quoted by him as having shown and published in the *Comptes-Rendus* for April 29th, 1850, that ice does not pass *abruptly* from the solid to the fluid state ; that it begins to *soften* at a temperature of 2° centigrade below its thawing point ; that consequently between 28·4 and 32° of Fahrenheit, it is actually passing through various degrees of plasticity within narrower limits, in the same manner that wax, for example, softens before it melts. M. Person deduces this from the examination of the heat requisite to liquefy ice at different temperatures. The

[7] Id. xl. 154-157.

following sentences contain his conclusions in his own words :
" Il parait d'apres mes experiences que le ramollissement qui précède la fusion, est circonscrit dans une intervalle d'environ 2 degrés. La glace est donc un des corps dont la fusion est la plus nette ; mais cépendant le passage de l'état solide à l'état liquide s'y fait encore par degrès, et non par un saut brusque." Forbes adds, that from his own experiments and those of Agassiz, it is clear that the normal temperature of the great mass of a glacier is between 28° and 32° Fahrenheit, that the coldest nights only affect the temperature of its superficial layers, that the lower parts which are habitually saturated with water in summer are seldom reduced below the freezing point, even by the prolonged cold of winter, since it is then covered with snow which has the property of preventing any profound congelation in common earth ; and as ice is probably a better conductor of heat than the ground it is incredible that a thickness of many hundred feet of ice, saturated with fluid water, should be reduced much below the freezing point, or should even be frozen throughout. The fact that water continually flows from under glaciers in winter shows that they are not so frozen. While the fact that, even in February, the source of the Arveiron becomes whitish and dirty as in summer before a change of weather, proves that in the middle of winter a temporary rise of temperature over the higher glacier regions, not only produces a thaw there, but finds the usual channels still open for transmitting the accumulated snow water. It thus appears quite certain that ice, under the circumstances in which we find it in the great bulk of glaciers, is in a state more or less *softened* even in winter ; and that during nearly the whole summer, while surrounded by air above 32°, and itself at that temperature, it has acquired a still greater degree of plasticity, due to the latent heat which it has then absorbed.[8]

His opponents continued, however, to appeal to the experience of hand specimens of ice, which are so brittle, and whose behaviour seems so remote from that of plastic bodies. In answer to them he writes :—

"I certainly never expected, when promulgating the

[8] Forbes " Sixteenth Letter on Glaciers," Ed. New Phil. Journ., Jan., 1851.

viscous theory, that it would have met with so much opposition on the ground that the more familiar properties of ice are opposed to the admission of its plasticity : and that the fragility of hand specimens should be considered as conclusive against the plastic effect of most intense forces acting on the most stupendous scale upon a body placed in circumstances which subject it to a trial, beneath which the most massive constructions of the pyramid building ages would sway, totter, and crumble. . . . in these days when the barriers of the categories are so completely beaten down, I had not expected to meet with so determined an opposition to the proposition that the stupendous aggregation of freezing water and thawing ice called a glacier, subjected to the pressure of thousands of vertical feet of its own substance might not under these circumstances possess a degree of yielding, moulding, self-adapting power, sufficient to admit of slight changes of figure in long periods of time. Still less could I have anticipated that when the plastic changes of form had been measured, and compared and calculated and mapped, and confirmed by independent observers, that we should still have had men of science appealing to the fragility of an icicle as an unanswerable argument. More philosophical, surely, was the appeal of the Bishop of Annecy from what we already know to what we may one day learn if willing to be taught. Quand on agit sur un morceau de glace, qu'on le frappé, on lui trouve une rigidité qui est en opposition directe avec les apparences dout nous venons de parler. *Peut-être que les expériences faites sur de plus grandes masses donneraient d'autres résultats."* [9]

In 1855, when Forbes wrote the article on glaciers in the *Enc. Brit.*, he spoke in very emphatic terms about his theory, which he said was then very generally accepted. In answer to·the obvious fact that ice is superficially a brittle solid, and does not seem readily plastic, he urged that sealing wax, pitch, and other similar bodies adapt themselves *with time* to the surfaces on which they lie even at atmospheric temperatures, while they maintain at the same time the quality of excessive brittleness under a blow or a rapid change of form. He went on to press that " ice does not pass

[9] Phil. Trans. 1846.

at once and *per saltum* from the solid to the liquid state, but absorbs its latent heat throughout a small range of temperature (between 28°·4 and 32° of Fahrenheit) which is precisely that to which the ice of glaciers is actually exposed, and that a glacier is not a crystalline solid like ice tranquilly frozen in a mould, but possesses a peculiarly formed and laminated structure, through which water enters (at least for a great part of the year) into its intrinsic composition. Putting together these facts, and admitting the differential motion of the parts, which no one now contests, the quasi fluid or viscous motion of the ice of glaciers is not a theory but a *fact;* a substance which is seen to pour itself out of a large basin through a narrow outlet, without losing its continuity, the different parts of which, from top to bottom, and from side to centre, possess distinct though related velocities, which moves over slopes inconsistent with the friction between its surface and the ground on which it rests—which surmounts obstacles, and even if cleft into two streams by a projecting rock, instead of being thereby anchored as a solid would necessarily be, re-unites its streams below, and retains no trace of the fissure, leaving the rock an islet in the icy flood; a substance which moves in such a fashion cannot in any true sense of the word be termed a rigid solid, and must be granted to be ductile, viscous, plastic, or semi-fluid, or to possess qualities represented by any of these terms which we may choose to adopt as least shocking to our ordinary conception of the brittleness of ice." It was no doubt with some satisfaction that Forbes quoted the words of Mousson, who in his *Die Gletscher der Jetztzeit,* p. 162, speaking of the plastic theory, says, " Er steht noch heute unangefochten da," while he numbered among his warmest supporters Darwin and Whewell.

While it was generally admitted that the phenomena accompanying the motion of glaciers had been shown by Forbes to exactly reproduce the conduct of a plastic body which can mould itself to its boundaries when in motion, and which has a continuous differential motion, the conduct of hand specimens continued to be a stumbling-block. Tyndall urged that a slight blow, if properly directed, will split open a block of ice ten or fifteen cubic feet in volume, and

as Mr. McGee says, "on a cold, still night, the steel runner of a boy's skate initiates a fracture miles in length in the ice bridging a river, which shows that if ice is plastic it is also very rigid under some conditions," but the fact which weighed most with observers was the experimental test applied by Professor Moseley, of Cambridge. His experiments were described in a paper read before the Royal Society in 1869, in which he published some very interesting results on the shearing force of ice. According to one experiment, the shear per square inch, or unit of shear was 72·433lb., and in another case 76·619lb., the mean being about 75lb., while, according to his calculations for a glacier to move by its own weight, as Tyndall had seen it move in the Mer de Glace, the unit of shear should not have been more than 1·319lb., whence he concluded that the weight of a glacier alone is insufficient to account for its descent."[1] These experiments had a very great effect on the scientific mind of Europe. They seemed conclusive against Forbes's views. As Mr. Trotter says, "granting that the shearing strength of ice as deduced from his (Moseley's) experiments represents even approximately the resistance to shearing under the actual circumstances of glacier motion, his objection to Forbes's view is fatal."

Mr. Moseley did not fail to use the weapon he had discovered, and pressed with great persistency the view that his experiments had proved the viscous theory to be untenable. On Mr Moseley's death, Mr. W. R. Brown, who championed his cause,[2] adduced fresh arguments against Forbes, from more general considerations. He urged that the existence of ice cliffs, such as those in the crevasses in South America, 300 feet thick, which ought to flow over if Forbes's view be right, seem to militate against it. Putting aside Forbes's own experiments about crevasses themselves being short-lived, the argument of Mr. Brown had been already answered by anticipation by Dr. Whewell. He says: "Soft pitch will stand in cliffs some inches high, soft clay will stand in cliffs many feet high; clay may stand in cliffs hundreds of feet high, and yet be plastic, if the mass be very large, and the

[1] Phil. Mag. xxxvii. 233-235.
[2] Proceedings of the Royal Society, vol. xxxiv.

pressure distributed through it be powerful enough to make one part move past another. We cannot doubt that clay might be hard enough to stand in such cliffs, and yet soft enough to slide down a sloping valley as a plastic substance, if the valley were filled with it for many miles long and hundreds of feet thick; and still more if there were streams of water running through all parts of the mass." [3]

Again, as Mr. Trotter says, "the spreading out of a glacier like the Rhone glacier when it emerges from a gorge on to a comparatively open space, is in itself a convincing proof that ice at 0° C. will not stand permanently in a vertical cliff of any considerable height. It gives way gradually, but still it gives way." [4]

These were, however, subsidiary issues. The main one converged upon Mr. Moseley's famous experiments, and no wonder that they led to a wide revolt against the plastic theory, and that a great impetus was given to various transcendental views, such as those of Thomson and Croll, already described, and when these were answered led to another suggestion, namely that of Tyndall.

Tyndall's most important pronouncement in criticism of Forbes's views is contained in the following sentence, published as an *obiter dictum*. " The very essence of viscosity," he says, "is the ability to yield to a power of *tension*, the texture of the substance, after yielding, being in a state of equilibrium, so that it has no strain to recover from." . . . He then goes on to urge that ice will not stretch like well-known plastic bodies, and while it yields to pressure it does not do so to tension.[5] " Viscosity," he says elsewhere, " consists in the power of being drawn out when subjected to a force of tension, the substance, after stretching, being in a state of molecular equilibrium, or in other words, devoid of that elasticity which would restore it to its original form." [6] He then goes on to urge that on dislocated slopes, where tar or treacle would flow without breaking across, ice breaks, and he cites certain cases in the Alps. He similarly refers to crevasses as breaks inconsistent with a viscous character, since the maximum strain upon the ice is comparatively so

[3] Phil. Mag., xxvi. 172 and 3. [4] Proc. Roy. Soc. xxxviii. 103.
[5] Proc. Roy. Inst., 1858, 551-553. [6] Glaciers of the Alps, 312.

small; and he further adds, that no single experiment on great masses or small shows that ice possesses in any sensible degree that power of being drawn out which seems the very essence of viscosity. Professor Tyndall carefully guards himself, however, against being supposed to believe that a glacier acts otherwise than conformably to the law of semifluid motion; all he denies is that ice is of a gluey tenacity, and will stretch like a plastic substance.

We will now turn to Tyndall's own theory. This theory which he applied to the motion of glaciers, was based upon a fact first published by Faraday in a lecture at the Royal Institution as early as 1855, according to which " when two pieces of ice with moistened surfaces are brought into contact they become cemented together by the freezing of the film of water between them, while, when the ice is below 32° Fahr., and therefore dry, no effect of the kind can be produced." [7]

" A generalization from this interesting fact," says Tyndall, "led me to conclude that a bruised mass of ice, if closely confined, must recement itself when its particles are brought into contact by pressure." [8] He therefore appeals to certain experiments which had, in fact, been made in a different way by others.

In a letter addressed to Forbes, on April 4th, 1846, by Mr. Christie, Secretary of the Royal Society, that gentleman instanced the experiment of filling a hollow iron shell with water, and exposing it to frost with the fuse hole uppermost, when the water will freeze, and protrude a cylinder of ice from the fuse hole, and as the experiment is continued the fuse continues to grow, in proportion as the water freezes. "No thawing takes place in the process. Does not this show plasticity even in very small masses of ice?" says Mr. Christie. The experiment was repeated by Forbes in glass vessels which could be closely watched. He first put a ring of a greasy matter of a red colour round the inside of the vessel, keeping the aperture clean. When the ice protruded it was found incased with red, in some cases in the form of a red ring, which, as he says, could not have been unless the plastic substance of the ice had been forced laterally, and, by a converging pressure from all sides, up even to the particles in

[7] See Glaciers of the Alps, 357.　　[8] Id.

contact with the interior of the glass, so as to be forced through the contracted outlet as a tenacious fluid under its own pressure, or a plastic solid subjected to a considerable force would do under like circumstances. The success of these experiments Forbes attributes to the slowness of the process of congelation employed, which lasted several hours, or, in Mr. Christie's case, several days, and which therefore afforded analogies with the gradual internal movements of a glacier.[9]

To turn to Tyndall's experiments, he tells us how in the course of them, moulds of various forms were hollowed out in boxwood, and pieces of ice were placed in them and subjected to pressure. In this way spheres of ice were flattened into cakes, and cakes formed into transparent lenses. A straight bar of ice, six inches long, was passed through a series of moulds augmenting in curvature, and was finally bent into a semi-ring. A small block of ice was placed in a hemispherical cavity, and was pressed upon by a hemispherical protuberance, not large enough to fill the cavity ; the ice yielded, and filled the space between both, thus forming itself into a transparent cup. In short, he says, every observation made upon glaciers, and adduced by writers on the subject in proof of the viscosity of ice, was shown to be capable of perfect imitation with hand specimens of the substance.[1] So far there was nothing on the surface to traverse Forbes's views in regard to ice being viscous, but the contrary. As Tyndall himself says " The experiments prove to all appearances that the substance is even much more plastic than it was ever imagined to be by the founders of the viscous theory "; but in the case of these experiments the inference would not have been quite correct. The process by which the ice was moulded into shape in them did not involve a continuous flow of semi-fluid particles over each other, thus adapting the ice to its new shape, but the crushing by force of the ice into an incoherent state, and then the process of refreezing it into a solid mass.

Tyndall himself describes it as a process of bruising and regelation, and denies that ice is viscous at all. On the

[9] The Theory of Glaciers, 161 and 168.
[1] Glaciers of the Alps, 321.   [2] Id.

contrary, he says emphatically, that a mass of ice at 32° is very easily crushed, but it has as sharp and definite a fracture as a mass of glass.[3]

I am bound to say that I can find nothing in the machinery by which glaciers are formed at all analogous to these experiments, except the initiatory stage, when damp snow is converted into continuous *névé* by pressure. From that point down to the foot of the glacier I can only find evidence of the ice being more and more condensed as the pressure is exerted upon it by its walls, until it acquires the character of blue ice. I can nowhere trace evidence of the pressure being sufficiently great, and such as to crush the ice into powder, or to assimilate it in any way to the disintegrated condition which it assumes in a Bramah press. When the pressure is the greatest, and the blue veins are produced, that is, when the great mass of the ice is detained by some projecting obstacle, there we have the least sign of disintegrated ice. On the contrary, there the ice is most transparent. Where the ice is embayed, and has therefore to spread itself and accommodate itself the most, there the signs of internal strain in the form of blue bands are conspicuously absent.

Similarly, if we examine the ice at the base of a glacier by entering one of the well-known ice caves, instead of finding its internal structure filled with cloudy lines, and more or less opaque, as Tyndall himself showed to be the case when incipient crushing is in progress,[4] we find it to be most blue and most transparent. On this matter Professor Bonney writes : " On entering ice caves, etc., underneath glaciers, where ice fills up cavities, the ice appears to enter these cavities not by fracture and regelation, but by change of form, as in the case of a plastic body, for it is remarkably clear and solid looking." [5]

Professor Tyndall himself allows that, according to his own view, " it is manifest that the continuity of the fractured surfaces cannot be completely and immediately reclosed after rupture. It is not the *same* surfaces that are regelated, and hence the coincidence of the surfaces cannot be perfect.

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[3] Id. 551.
[4] Glaciers of the Alps, 409 and 10.   [5] Journ. Geol. Soc. xiii. 199.

They will enclose for a time capillary fissures,"[6] but the existence of such capillary fissures in the deep ice is absolutely denied by Professor Huxley, who accompanied Professor Tyndall to Switzerland.

Professor Huxley's words are : "All deep ice, that is, all ice situated more than a few inches below the surface, is as solid as glass or marble and as devoid of any but accidental fissures. The glacier, however, where exposed to the atmosphere, presents what may be called 'a superficial layer' of very different character. It is composed of larger or smaller granules of exceedingly irregular form, separated by very obvious fissures, but nevertheless so fitted into one another as to cohere with firmness. The thickness of the superficial layer varies a good deal, seven or eight inches being rather above the average depth. Wherever you clear away this superficial layer you find beneath it what I have termed deep ice, that is, ice in which neither fissures nor granules are visible." Professor Huxley did not content himself with a microscopic examination of glacier ice, but he tested its porosity by making cavities, some of them with very thin walls, and filling them with coloured liquids, and thus testing its permeability; and he says, "I can only conclude from these experiments that the chief substance of a glacier is as essentially impermeable as a mass of marble or slate; and that, though it may be traversed here and there by fissures and cracks, these no more justify us in speaking of glacier ice as porous than the joints and fissures in a slate quarry give us a right to term slate porous. We do not call iron porous because water runs out of a cracked kettle."[7]

This conclusion of Professor Huxley seems absolutely fatal to Professor Tyndall's conclusion. As Dr. J. Ball says, "If we are to adopt his conclusion, then we must cease to believe with Professor Tyndall, that glacier ice is enabled to advance in conformity with the law of viscous motion, by fracture and regelation."[8]

When we turn from these to another series of facts, whose lesson is ignored by Professor Tyndall, namely, the proved differential motion by which every point from the centre of a

[6] Proceedings Royal Inst., 1857, p. 322 note.
[7] Phil. Mag. 4th ser. xiv. 250.   Id. 502.

glacier to the side, from its summit to its base has a different motion, we have a condition which seems to me compatible only with semi-fluid motion, and utterly unexplainable by any theory of crushing and regelation.

The same thing seems to follow from one of Tyndall's own discoveries, namely, that the thread of the ice current shifts its position to the right and left of the medial line, and approaches each side in turn. Thus the axis of movement follows an undulating line, the windings of which are more decided even than those of the gorge of the glacier.[9] This is exactly consistent with the motion of liquid and viscous bodies.

Referring to Moseley's argument on the shearing force of ice, Mr. Trotter says :—" It seems to be decisive against the belief that the ordinary comparatively undisturbed motion of a glacier along a moderately sloping bed takes place by fracture and regelation. Moseley's value of the shearing strength of ice, which has been shown to be enormously too great as a measure of the resistance of ice to slow shearing, would appear on the other hand to be an inferior limit to the resistance to the shearing fracture which must precede regelation." [1]

Again, Professor Tyndall refers to experiments of his own upon the Mer de Glace, showing its motion in winter when the estimated temperature was 5 degrees below zero centigrade. How, at such temperatures, could regelation occur at all ? The ice surfaces would be all dry and incapable of freezing together. We must remember that it is only damp surfaces that will freeze together again. Professor Tyndall acknowledges that below 32° ice does not possess the property of regelation, and it has been proved that a glacier is always moving in all its parts in winter as well as in summer, below as well as above. It means that the interior of a glacier must, in order to justify Professor Tyndall's theory, be above the freezing point or at the freezing point in winter, which is completely contrary to the facts as we know them, in the glaciers of Greenland and high latitudes, which have been shown to have a considerable winter motion.

[9] Reclus, The Earth, i. 202.
[1] Proc. Roy. Soc. xxxviii. 108.

Heim urges that Tyndall's theory also fails when tested by the internal crystalline nature of glacier ice. He proved no doubt that damp crushed ice or snow will by regelation form a solid mass of ice, but not a solid mass of ice composed of crystals like a glacier is.[2]

The fact that in some cases when objects have been engulphed in glaciers, they have been widely scattered and not crushed together, seems to again point to the internal condition of glacier ice being one of tension rather than of mutual pressure.

In a paper in the *Alpine Journal* on some relics of guides lost on Mont Blanc, we read how they were found at the lower end of the Glacier des Bossons, and have been identified as the remains of some guides lost on August 20th, 1820. There were found a piece of paper, a fragment of an alpenstock, a piece of bottle glass, a large portion of the iron frame of a lantern, and a small bit of skull. They were all embedded in the ice near the bottom of the glacier. They must have travelled $4\frac{7}{8}$ miles if they went in a straight line, and Mr. Cowell calculated that taking other things into consideration, their whole journey would be $5\frac{1}{2}$ miles, *i.e.* 225 yards for 43 years, or $22\frac{1}{8}$ inches a day. The relics were scattered over a space 50 feet square. If they were being constantly compressed, why were they not closer together? The fact is more remarkable because the guides were overwhelmed by an avalanche which was composed of snow, and as about three feet of snow compress into one of ice, we ought to find the three men or each man singly, compressed up into a small lump with all his things, but this was not so. They were torn to pieces and scattered. All round them the ice was covered in every direction for twenty or thirty feet, with hair from one knapsack spread over an area 300 or 400 times greater than that of the knapsack. Mr. Cowell says, he had on another occasion seen on the St. Théodule glacier at the foot of the Breithorn, the remains of the Syndic of Val Tourranche and his mule scattered over several acres.[3]

These considerations seem fatal to Professor Tyndall's theory, and its breakdown made men revert again to Forbes's

[2] Op. cit. 319.
[3] Alpine Journ. i. 332-338.

views, notwithstanding the apparently insuperable objection contained in Moseley's experiments. Among these champions of the Scotch philosopher was Dr. M. Williams. "We have direct evidence that ice of great thickness," he says, " actual glaciers, may bend to a considerable curvature before breaking. This is seen very strikingly when the uncrevassed ice-sheet of a slightly inclined *névé* suddenly reaches a precipice and is thrust over it. If Mr. Geikie were right (*i.e.* if ice were a rigid and not a plastic body) the projecting cornices thus formed should stand straight out, and then when the transverse strain due to the weight of this rigid overhang exceeded the resistance of tenacity, it should break off short, exposing a face at right angles to the general surface of the supported body of ice. . . . Some very fine examples of such ice cornices are visible from the ridge separating the Handspikjen Fjelde from the head of the Jostedal, where a fine view of the great *névé* or sneefornd is obtained. This side of the *névé* terminates in precipitous rock walls; at the foot of one of these is a dreary lake, the Styggewand. The overflow of the *névé* here forms great bending sheets that reach a short way down, and then break off and drop as small icebergs into the lake." [4]

Kane, speaking of the Greenland ice, says : " It seemed as if a caldron of ice inside the coast ridge, was boiling over, and throwing its crust in huge fragments from the overhanging lip into the sea below. The glacier must have been 1100 feet high, but even at its summit we could see the lines of viscous movement." [5]

Again, he says, of Tyndall's glacier : "I have never seen one that illustrated more beautifully the viscous, or semi-solid movement of these masses. It had two planes of descent, the upper nearly precipitous for about 400 feet from the summit, the lower communicating by a slightly inclined platform, perhaps half a mile long. This ice was unbroken throughout. It came down from the level of the upper country, a vast circle with the folds or waves impressed upon it by its onward motion, undisturbed by any apparent fracture or crevasse. Thus it rolled onward, over the rugged and contrasting plat-

[4] Quart. Journ. of Science, vii. 220-21.
[5] Arctic Explorations, ii. 259-60.

form below, and thence poured its semi-solid mass down upon the plain. Where it encountered occasional knobs of rock it passed round them, bearing still the destructive marks of an imperfect fluid obstructed in its descent : and its lower fall described a dome, or, to use the more accurate remark of Forbes, ' a great outspread clam-shell of ice.' In many places the ice could be seen exuding or forcing its way over the very crest of the rocks, and hanging down in huge icy stalactites seventy or one hundred feet long. These were still lengthening out by the continuous overflow, some of them breaking off as their weight became too great for their tenacity, others swelling by constant supplies from the interior." [6]

Speaking of the great Humboldt glacier, Kane describes it as " moving onward like a great glacial river, seeking outlets at every fiord and valley, rolling icy cataracts into the Atlantic and Greenland seas ; and having at last reached the northern limit of the land that has borne it up, pouring out a mighty frozen torrent into unknown Arctic space. . . . Here was a plastic moving, semi-solid mass obliterating life, swallowing rocks and islands, and plowing its way with irresistible march through the crust of an investing sea. . . . The ' overflow,' as I have called the viscous overlapping of the surface, was more clearly marked than upon any Alpine glacier with which I am acquainted. When close to the island rocks, and looking out upon the upper table of the glacier, I was struck with the homely analogy of the batter-cake, spreading itself out under the ladle of the housewife. The upper surface less affected by friction, and rolling forward in consequence. . . . The point at which this immense body of ice enters the land of Washington, gives even to a distant view impressive indications of its plastic or semi-solid character. No one could resist the impression of fluidity conveyed by its peculiar markings ; I have named it Cape Forbes after the eminent crystallologist, whose views it so abundantly confirms." [7]

The position in which the problem now stood was remarkable. It was admitted by everybody that the general phenomena of the motion of a glacier exactly reproduces that of a viscous body moving through a channel under the influence of its own

[6] Id. i. 334-36.
[7] Id. i. 228, and ii. 146-53.

weight, and that Forbes's view seemed incontrovertible, and every attempt to explain the motion of glaciers by other processes had failed. But for the experiments of Moseley, which seemed to show that the shearing resistance of ice is not sufficiently low to allow us to treat a glacier as a viscous mass, and that consequently gravity is not strong enough to do the work demanded from it, there would have been no hesitation among scientific men in accepting the plastic theory as alone valid.

It had not occurred to inquirers to call in question Moseley's experiments. This was now to be done, with the result of completely vindicating Forbes's view.

Mr. Ball objected, on theoretical grounds, to experiments like Moseley's upon artificially formed ice being applied to ice of another kind altogether, namely, glacier ice, a conclusion for which, as we shall see, he had full warrant. He also urged that while the shearing in a glacier, whatever its amount, takes place very slowly, in Mr. Moseley's experiment it was very fast. Moseley's own experiments show, that if we want to shear ice quickly, a weight of 120 lbs. is required, while, if the thing is done more slowly, 75 lbs. will suffice, and this gives point to Mr. Ball's criticism, that to ascertain the resistance opposed to very slow changes in the relative position of the particles, so slight as to be insensible at short distances, Mr. Moseley measures the resistance opposed to rapid disruption between contiguous portions of the same substance.[8]

Mr. Matthews and Mr. Reilly, by careful experiments, emphasized this argument of Mr. Ball, and showed how important an element time is in the shearing of ice; the actual differential motion of a glacier ranging for molecules the tenth of an inch apart, and at an interval of 24 hours, from the $\frac{1}{2130}$ to the $\frac{1}{70000}$ of an inch, while Mr. Moseley inferred the shearing force to be 75 lbs., from experiments in which he caused a solid cylinder of ice to shear an inch in half an hour.[9]

It was in 1870 that Mr. Matthews tried his famous experiment upon the shearing of ice, which gave an entirely

[8] Phil. Mag. xl. 158.
[9] Id. xlii. 420.

different result to that of Mr. Moseley. A plank of ice 6 inches wide and $2\frac{3}{8}$ inches thick, was sawn from the frozen surface of a pond, and supported at each end by bearers 6 feet apart. The whole weight of the plank could not have exceeded $37\frac{1}{2}$ lbs, and its cross-section was nowhere less than 14 inches. From the moment the plank was placed in position it began to sink, and continued to do so until it touched the surface over which it was supported. At the point of contact it appeared bent at a sharp angle, and was perfectly rigid in its altered form. The total deflection was 7 inches, which had been effected in about as many hours, under the influence of a thaw, during which the plank diminished very slightly in thickness. It was thus shown that ice can change its form under strains produced by its own gravitation.[1]

The meaning of the experiment was very plain. Mr. Moseley himself says of such bending, " When the ice takes a distinct set, every particle, except those at the points of support, is made to move in the direction in which the plate is bent, those particles which are at the point of greatest inflation being made to move furthest, and those nearer to it being always made to move further than those most remote, so that every particle moves over that which is alongside towards the nearest point of support; and being assumed to have taken a set, it must have sheared over it." That is to say, the motion of the plank is precisely such a motion as Forbes's theory requires. Mr. Matthews pertinently asks :—If the shearing force of ice be 75 lbs. as Mr. Moseley requires, and the cross section of the ice plank is 14 inches, it must require a pressure of 1050 lbs. in order to shear it. How then does it come about that it shears under a pressure of $37\frac{1}{2}$ lbs ? The experiments showed that even if the whole weight of the plank were at work in shearing it only along the supporting edges, instead of a portion of it being taken off by the bearers and being otherwise extended, it could not exceed $37\frac{1}{2}$ lbs. for the whole area, which, for the two surfaces, would be $\frac{37\frac{1}{2}}{28}$, or about $1\frac{1}{3}$ lbs. to the square inch.

[1] Alpine Journal, 1870, 426.
[2] Phil. Mag. xlii. 333-4.

In March, 1871, Professor Bianconi published a paper in the *Memoirs of the Bologna Academy*, Series 3, vol. i. p. 156, entitled, "Esperiénze intorno alla flessibilità del Ghiaccio." In this paper he refers to experiments which he had tried from 1866 onwards upon beams of ordinary ice suspended on points and weighted in between. In these experiments he found that the beam acquired a curved flexure, and when it was reversed, and the weight again attached, it curved in the opposite direction. In another set of experiments he showed that after some hours a beam of ice is susceptible of torsion. In other experiments which he tried with granular ice, formed from snow, the results were more marked. To his memoir are attached plates in which the amount of bending and torsion are shown. The general result is summed up in the following words : "Conchiudo, il ghiacco possiede una flessibilita o pieghevolézza assai lente, ma bene spiegata, alla temperatura di + 2, + 3 etc. mentre rimane in ogni momento presente la somma ma fragilita."[3]

In the fourth volume of *Nature*, Professor Tyndall published some experiments he himself made on ice from the Morteratsch Glacier in 1871. In these experiments it was clearly shown that a stout rectangle of clear and continuous ice cut from a glacier, when properly supported and weighted, showed signs of bending after twelve hours. Similar experiments were made upon bars of ice from the harder and firmer ice of the sand cones. When not too large these showed similar signs of bending very quickly, and the flexure in each case was *permanent* and not due to elasticity.[4]

More remarkable were the experiments of Mr. John Aitken, described in the seventh volume of the same periodical (287 —288), from which he deduced the conclusion that the plasticity of ice depends largely on the amount of air it contains. Having dissolved a large quantity of air in water, he filled some tubes with it. When frozen he withdrew the ice in the form of rods, which he then placed on supports eight and a half inches apart, and hung a pound weight in between. The beams, he says, at once began bending, and continued bending so long as the weights were left on them, thus proving the viscosity of the ice. In further experiments

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[3] Op. cit. 165.   [4] Nature, vol. iv. 447.

with rods made from compressed snow, afterwards frozen in a freezing mixture, and therefore more like glacier ice, the results were even more marked, one of the beams bending an inch in five minutes. Eventually Mr. Aitken succeeded in twisting such ice rods round cylinders, and thus forming solid ice rings from straight beams of ice.

In 1877 Professor Pfaff published the results of some careful experiments, which went to show that even the slightest pressure when applied continuously, and when the temperature of the ice is near the melting point, is sufficient to displace the particles of ice. "It follows," he says, "that ice near its melting point behaves *indeed like wax.*" Again he says, "It is still constantly assumed, on the ground of some of Tyndall's experiments, that ice is destitute of *extensibility* and *flexibility*, although repeated observations recently made compel us to ascribe to it some flexibility. The oldest observation of this kind known to us originated with Kane, who remarked that a large lump of ice with its edges resting on two others became curved in the course of some months." Professor Pfaff then details some experiments confirming this one, and like others I have already quoted. "I next endeavoured," he says, "to determine the amount of extension of ice by *traction*, and he attached a weight to a prism of ice. After seven days, signs of stretching were clearly visible. It is therefore shown," he says, "that a pull continued for a long time, even when it is slight, stretches ice ; that near its melting point it shows itself like other bodies yielding to pressure as well as to pull ; and at a temperature in the vicinity of zero, it is to be regarded as an eminently plastic substance."[5]

In 1883 Mr. C. Trotter tried some experiments on the shearing of ice in an ice grotto at Grindelwald, the apparatus being placed about eighteen metres from the edge of the glacier, twenty-five or thirty metres below its upper surface, and about the same above its bed, so as to have conditions of temperature like those of an actual glacier, and the ice used was cut from the glacier itself. The result was to show that under a force rather more than double that which, according to Canon Moseley's calculations, is

[5] Phil. Mag. 1. 333-36.

exerted by gravity in the Mer de Glace, near the Tacul, but $\frac{1}{25}$ only of his smallest value of the shearing force of ice, the amount of shear was actually larger than that implied in any of the ordinary cases of glacier motion, and he concludes that there is little doubt that under conditions closely resembling those of the interior of a glacier, and under the influence of forces comparable with those of gravity, hand specimens of ice shear in the same manner as a truly viscous solid would.[6]

The observations of Tyndall upon crevasses do not prove that ice is not extensible, but that it is incapable of any appreciable elastic extension before it gives.

" I believe, therefore," says Mr. Trotter, " that the weight of evidence tends to show that ice at or about 0° C. is just as truly viscous as pitch or sealing-wax at temperatures at which they are brittle, but yet capable of yielding to the continuous application of a very moderate force. The viscosity of ice, however, probably diminishes very rapidly with the temperature. . . . This is in complete accordance with the facts of the changes which take place in a glacier during the winter. The terminal melting ceases, but the advance of the end of the glacier into the valley is very slow, and probably ceases altogether in the depth of winter. Higher up, the forward movement of the surface continues, though at a slower rate than in summer, and though the glacier does not lengthen much in winter, it thickens considerably, and the surface rises, often through many feet, so as to make up the enormous waste of the summer."

In May, 1887, Dr. Mann read an important paper before the Royal Society upon some experiments he performed in the Engadine upon the shearing of ice. These experiments, as he tells us, in order to eliminate the influence of regelation, were carried on at low temperatures, the *highest* being $-2\cdot6°$ C., number 2 $-1\cdot0°$ C., and number 3 $-0\cdot5°$ C. The ice was frozen in a cylindrical mould, and in order to exclude air the water was boiled. Without giving the details, the result obtained was that " ice subjected to tension stretches continuously by amounts which evidently depend on the temperature and on the tension stress.

[6] Proc. Roy. Soc. xxxviii. 1000-11.

When the stress was great, and the temperature not very low, it amounted to as much as one per cent. of the whole length per day. When the temperature was lower and the stress was less, the extension was less, but still such as could be measured. So continuous and definite was the extension, that it could even be measured from hour to hour. . . . Hence differential motions resulted in the ice. These motions and extensions took place at temperatures which preclude all possibility of melting and regelation. . . . That there is such extension, and that it goes on continuously with all stresses above one kilo. per square centimetre, and at all temperatures between —6° C. and the freezing point, is shown by the above experiments. . . . In the discussion, for the most part *a priori*, on the extensibility of ice, sufficient importance has not usually been assigned to the necessity of distinguishing between the effect of even a small blow or jar, and that of a much greater force applied gradually and steadily during a long interval. A bar of ice may bear a stress of four and five kilos per square centimetre if the load is steady, which would fracture at once with a much smaller sudden stress, especially if not uniformly distributed." [7]

In the following year similar experiments were repeated by Messrs. McConnell and Kidd, " who established," to use their own words, " that not merely the rate, but even the very existence, of the extension, depends on the structure of the ice." They showed that when the ice consists of a single crystal, hardly any extension takes place, thus, *pro tanto*, explaining the results of some of the earlier experiments. Thus clear ice cut from the surface of a bath, which proved to be a rough regular crystal, showed hardly any extension; ice of irregular structure made in a mould, showed considerably more. This made it obvious, that for the purpose of testing the question as applied to the motion of glaciers, actual glacier ice must be used. " And the experiment was next tried upon actual glacier ice, taken from the natural caves at the foot of the Morteratsch Glacier." To use the very words of the experimenters :—

" We tested three pieces, which were quite sufficient to disprove the common notions, that glacier ice is only plastic

[7] Proc. Roy. Soc. xlii. 491-501.

under pressure not under tension, and that regelation is an essential part of the process. They showed at the same time the extreme variability of the phenomenon. The first extended at a rate of from 0·013 mm. to 0·022 mm. per hour per length of 10cmm., the variation in speed being attributable to the temperature. The second piece began at a rate of 0·016 mm. and gradually slowed down till it reached, at the same temperature, a rate of 0·0029 mm., at which point it remained tolerably constant, except for temperature variations, till a greater tension was applied. The third piece, on the contrary, began at the rate of 0·012 mm., increased its speed with greater tension to 0·026 mm., and stretched faster and faster with unaltered tension till it reached the extraordinary speed of 1·88 per hour per length of 10 cmm. We put on a check by reducing the tension slightly, whereupon the speed fell at once to 0·35 mm., and gradually declined to 0·043 mm. . . . During twelve hours, with a maximum temperature −9° and a mean temperature probably −10·5, the rate under the light tension of 1·45 kilo. per square cm. was 0·0065 mm." . . . " We tried further experiments on compression of ice, the pressure being applied to three nearly cubical pieces at once. Of three pieces of glacier ice, under a pressure of 3·2 kilos per square cm., the mean rates of contraction during five days were respectively 0·035 mm., 0·056 mm., and 0·007 mm. per hour, per length of 10 cmm. These figures show that while the plasticity varies enormously in different specimens, the rate of distortion is of the same order of magnitude, whether the force applied be a pull or a thrust. . . . We have now shown by direct experiment that ordinary ice, consisting of an irregular aggregation of crystals, exhibits plasticity both under pressure and under tension, at temperatures far below the freezing point—in the case of tension at any rate down to − 9° at least, and probably much lower. . . . It will be interesting to make some comparison between the figures we have given and the plasticity actually observed in the motion of glaciers. Perhaps the most striking proof of the existence of plasticity is the great increase of velocity from the side to the centre of a glacier. The most rapid increase mentioned by Heim [8] among the glaciers of the

[8] Gletscherkunde, 147.

Alps is on the Rhone glacier on a line 2,300 metres above the top of the ice fall. At 100 metres from the Western bank, the mean yearly motion, from 1874 to 1880, was 12·9 metres; at 160 metres from the bank it was 43·25 metres. This gives an increase of velocity in each metre across the glacier of 0·00058 metres per hour." Having calculated out what this means, our authors proceed : "Thus the maximum rate of extension in the case we have taken on the Rhone glacier is 0·0029 mm. per hour per length of 10 cmm. This, be it remembered, is the most rapid extension selected from a large number of measurements on different glaciers and at different times, and yet only one of the three specimens of glacier ice showed a rate less than this, and that was under one-third of the breaking tension. The larger the specimen the greater average plasticity would it display."

In some still more recent papers read before the Royal Society by Mr. Thomas Andrews, he shows experimentally that the shearing force of ice is largely dependent on its temperature. "In the majority of instances," he says, "it was found that if the plasticity of the ice at -35 Fahr. be called 1 at 0' Fahr. it would be about twice as much, and at 28 Fahr. the plasticity would be about four times as great as at 0' Fahr., or eight times as much as at -35 Fahr. . . . . This is in accord with the practical cessation of motion in glaciers during the cold of winter. It was also noticed that the plasticity of the naturally frozen pond ice was manifestly greater than that of the prepared pure ice."

These experiments have shown conclusively that Mr. Moseley's tests which misled so many scientific men were based on a mistake, and with the disappearance of Moseley's experiment, disappears the only evidence that has been forthcoming against the splendid induction of Forbes.

We may take it, therefore, as clearly proved, that glacier-ice is not a rigid body, but a plastic one; and that its movements may be compared with those of pitch or other plastic substances, whose several parts can roll over one another. When ice moves under the influence of gravity, except on very rapid slopes, it acts like other plastic substances act. Its lower surface, in contact with the ground, is dragged by friction, and moves very little, while its upper part flows

faster. If we pour pitch on a table, we find that it spreads out, not by the bottom of the mass spreading, but by the edges rolling over; the upper stratum curling round to form the lower one, which is dragged by the surface of the table. Just as a drop of water rolls down a plain, leaving in its track the successive bottom layers of itself.

In claiming ice as a plastic substance I do not mean that it is completely plastic, but that it behaves like sealing-wax and other similar bodies, which mould themselves with time to the surfaces on which they lie, even at moderate atmospheric pressures, and maintain, meantime, the quality of excessive brittleness under a blow or rapid change of form. The very fact of its cracking and forming crevasses shows that it is not perfectly plastic, but under certain conditions of tension will snap like a brittle substance. Its viscosity doubtless also varies both with its temperature, as Forbes urged, and also with the character of its molecular structure; and we may conclude as the result of our inquiry, that the motion of a glacier is due in the main to the actual flow of its substance, which goes on continuously, and, secondly, to a certain slight sliding over its bed, and certain more sudden movements due to large masses cracking asunder under great tension, the first being no doubt much the most potent and influential of these causes. So far as we can judge, none of its motion is due to molecular movements other than those induced by gravity. If ice were contained in a basin, like water in a lake, or spread out on a level plain, it would neither crack nor move unless thrust out by external pressure, and such pressure in nature can, so far as we see, only be derived from gravitation.

As Mr. Trotter puts it : " The fuller consideration of the physical properties of glacier ice leads to essentially the same conclusions as those to which Forbes was led forty-one years ago, by the study of the larger phenomena of glacier motion, that is, that the motion is that of a slightly viscous mass, partly sliding upon its bed, partly shearing upon itself under the influence of gravity." [9]

This conclusion is a very important one. It displaces a great deal of ingenious, and in some cases transcendental

[9] Proc. Roy. Soc. xxxviii. 107.

reasoning on the nature and phases of ice, with which the writings of very distinguished men have been sophisticated in the last quarter of a century ; and, in my view, it effectually disposes of the theories of the excavating powers of ice, and of the motion of great ice sheets which the current school of glacial geologists has imposed on the credulity of men of science.

To a discussion of the former of these theories I propose to devote the next chapter.

CHAPTER XIV.

THE ALLEGED POWER OF ICE TO EXCAVATE VALLEYS, CIRQUES, FIORDS AND LAKES, CONFRONTED WITH THE LAWS OF PHYSICS.

"I have a strong impression that the glacial theory is now at about its maximum, and that when all our valley systems are described as being nothing but magnified striæ, we are pretty near the summit level of this particular excursion of the scientific imagination."—The Duke of Argyll, Q. J. G. S., xxix. p. lxxviii.

Limitations of the motion of a glacier when moving *en bloc*—The modulus of cohesion of solid ice—Mallet, Oldham—Various causes which reduce the effects of gravitation upon a glacier—Irving— The mechanical effects of ice upon its bed—Whymper, Dawson— Ice mixed with stones, not a firm tool, but an unstable one— Spencer—The originators of the ice erosion theory—Dana, Ramsay, and Tyndall—Ramsay's and Tyndall's arguments mutually destructive—Erosion of valleys by ice—Tyndall, his views criticized by Ramsay, Ball, Whymper, Bonney, the Duke of Argyll, J. C. Russell —Erosion of cirques by ice, Wright, Bonney, Gastaldi, Holland, J. C. Russell, Goodchild, Geikie, and Jukes Brown—The glacial erosion, of fiords, Dana, Ramsay, Brown, Murphy, Murchison, Taylor, Pettersen, Falsan, Viollet le Duc—Glacial erosion of lakes— Ramsay, Whymper, Lyell, Murchison, Collomb, C. Martins— Reply of Ramsay to his critics—Renewed criticism—W. J. Hamilton, Ball, Irving, the Duke of Argyll, Bonney, Judd, Favre, Falsan, Spencer.

In the previous chapter I claim to have shown that the only theory which explains all the facts of ice motion, is that which treats ice as a more or less viscous body, which moves partly *en masse* in its bed, and partly, and in a much more important way, by a differential motion among its particles as other viscous substances do. In both cases the impelling force and cause of the motion is gravity, and gravity alone. Where gravity cannot operate, when there is no pressure either from the glacier's weight acting on it *en masse*, or acting differentially on its particles, in consequence of the ice having a sloping surface, it will not move, but will remain as sta-

tionary as ice in a pond. This is a most important and far-reaching conclusion.

I propose, in the first instance, to apply it as a test to the theory that ice is an excavating agent. The movement of a glacier *en masse* under the influence of gravity need not delay us. It is strictly limited to its motion on a sloping bed, and the rate of progression depends very much on the slope of the ground, and the mass of the glacier, it being a function of these two elements, partially interfered with by the roughness of the ground over which it has to travel, the sinuosity of its channel, and other factors which make up the friction which it has to overcome.

It seems quite clear that a glacier moving in this way cannot move far from the slope which gives it its original impulse. It can move no further than the thrust of the ice behind it will take it. This thrust has certain very narrow limitations.

Mr. Mallet stated at a meeting of the Geological Society that experimental data were, as yet, wanting to enable a precise determination of the limit of distance to which an extraneous force could be transmitted through a prismatic mass of ice. The fundamental point of such an inquiry is, what is the modulus of cohesion of the most solid ice? A few experiments had been made, which showed that the height of this modulus could not exceed a few hundred feet. "Let it be assumed, however, that it is as great as 5000 feet, or a mile. It is then obvious that a mass of ice, no matter how deep or wide, lying in a straight, smooth, frictionless valley, cannot be pushed along by any extraneous force, in the line of the valley, through a distance of more than a single mile, for, at that point the ice itself must crush, and the direct force cease to be transmitted further. This, of course, is far from being the whole of the question of the transmission of force through ice, for when and wherever crushing takes place, a certain portion (though a small one) of the direct pressure is transmitted laterally by the crushed fragments, especially if mixed with water. For this to take place, however, in the direction of the length of the ice-filled valley, supposes the ice must be considerably more than a mile in vertical depth." This argument was carried further by Mr. Oldham, in a paper on the

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[1] Q. J. G. S., xxix. 223.

modulus of cohesion of ice and its bearing on the theory of glacial erosion of lake basins. He adduces experiments to show that under a pressure of cohesion of its own substance, 370 feet high, ice must yield, and that no slow pressure greater than an equivalent to that depth of ice, could be transmitted by ice, while under a pressure of 960 feet of its own substance ice would be crushed.

Taking the modulus of ice at 1000 feet, or three times what it is actually seen to be, taking the angle of repose of clean ice on moderately rough sandstone, as determined by Mr. Hopkins, to be 20°, and the co-efficient of friction at only 2°, (the co-efficient of friction must be greater than tan 20°, or ·577, otherwise, since a glacier has embedded in its substance quantities of rock, there would be no erosion), it follows that in order to excavate the lake of Geneva, whose extreme length is 45 miles, its extreme depth, 984, say 1000 feet, and the distance of the greatest depth from the lower end of the lake, 25 miles, giving a slope into the lake of 33′, and out of the lake of 26, both being taken as uniform, it would require a pressure of over nine miles of ice to force a glacier *en masse* through and out of the lake. " Compare this," says Mr. Oldham, " with the observed modulus, and further comment is superfluous." He then goes on to show that a glacier 5000 feet in depth at the head of a lake basin, and thinning off to 2500 feet at its foot, could not scoop out a lake of more than 700 feet in depth under any circumstances whatever, nor could it indeed scoop one out of that depth. This is the very outside limit of what a glacier could possibly do, and holds good on any theory of glacier motion, and he concludes that no lake basin exceeding 700 feet in depth, or five miles in length, could possibly owe its origin to glacial erosion, though the true limits are probably not one-tenth of these quantities. He adds that Mr. R. Mallet authorized him to state, that according to the experiments made by Professor Phillips, the modulus of ice is reduced to almost nothing by the pressure of interstitial moisture; so that if, as is generally supposed to be the case, glacier ice is permeated by interstitial moisture, the power of a glacier inscooping out a lake basin would be reduced to almost nothing." [2]

[2] Phil. Mag., 1879, 240-247.

Mr. Oldham further proves that the resistance which would be offered to a glacier, moving as a whole through any depression that might be in its path, would increase as the slope of the depression diminished, thus the resistance for an angle of 1″ would be ten times that for an angle of 10′, for an angle of 10′ about ten times that due to a slope of 1° 40′. This is very important, for the advocates of the ice-erosion of lakes argue that since they are of insignificant depth, compared with their length, the erosion of lakes would be easy, the fact being that their very shallowness makes the excavation of lake basins of any great size by this means, not only improbable, but absolutely impossible.[3]

"The greatest distance," says Mr. Oldham, "to which a glacier could be forced *en masse* is about five miles, so that a glacier debouching on a plain could not exert any erosive power on that plain for more than five miles from the commencement of its level course, and consequently, could not scoop out a lake basin of more than that length, whatever its depth might be."

Apart from this initial difficulty, in which the problem has been stated in its favourable aspects, it must be remembered that only a very small part of the force of gravity acting upon a glacier is available for excavating purposes. "As it is," says Mr. Irving, "the forward thrust is, to a very large extent, resolved into an indefinite number of smaller forces, which are expended, either directly or indirectly, in overcoming cohesion. It follows at once from this, as a simple deduction from the law of the conservation of energy, *that the residuum of energy available for any supposed excavating action of a glacier is comparatively small.* . . . The greater part of the forward thrust of the glacier-mass is expended in overcoming cohesion and in causing movements among the parts of a glacier relatively. . . . Another way in which some part of the potential energy due to weight is expended, is in the formation of crevasses. There is an expenditure of a portion of the weight of the ice mass below each crevasse, which is quite unavailable for purposes of erosion. Again, in the formation of the well-known *Bergschrund*, the ice below it having torn itself away from the *nevé* above it, the weight of the latter is no longer

[3] Id. 245.

capable of co-operating with the weight of the ice below it.
. . . The consequence of the creation of crevasses is, in such
cases, a breaking-up more or less of the glacier mass, and the
consequent distribution of its force as a moving body." We
can readily see, therefore, that a considerable amount of the
force of gravity contained in a glacier is used up within the
glacier itself, and is not available either to give it a forward
thrust along a horizontal surface or for eroding purposes.

We must also remember that a part of the actual motion of
the glacier may promote sliding by diminished friction, and
therefore be again less available for erosion. As (1) the non-
contact of the ice with the rock, in places where glacier
streams are flowing; (2) the thermal effect of the heat flowing
from the earth's crust by conduction from below; (3) com-
paratively warm water rising in places from deep-seated
springs; (4) the partial liquefaction of the ice by pressure
against the rocks. All these, by diminishing friction,
dispense with some portion of the pushing force due to
gravity; so that the proportion of that used up within the
mass of the glacier is even much greater than the estimate
alone would give.[4] This being the *a priori* objection to the
glacier erosion theory, let us now examine it a little more
concretely.

Whymper was the first person who sifted the *modus operandi*
of a glacier, when eroding its bed with accuracy and care, and
I shall quote liberally from him. He commences by referring
to an observation of Studer, that glacier erosion differs from
that of water in producing convex rather than concave sur-
faces, and continues :—

" When water is in the form of a glacier, it has not the power
of making concavities and of working upon surfaces which are
not opposed to the direction of the current. . . . It leaves
marks which are readily distinguished from those produced
by torrent action. The prevailing forms, which result from
glacier action, are more or less convex. Ultimately, all
angles and almost all curves are obliterated, and large areas
of flat surfaces are produced. The perfection of abrasion is
rarely found, except in such localities as have sustained a
grinding more severe than that which has occurred in the

4 Q. J. G. S., xxxix. 62-66.

Alps. . . . Not merely can the operations of extinct glaciers be traced in detail by means of the bosses of rock, popularly termed *roches moutonnées,* but their effects in the aggregate on a range of mountains or an entire country, can be recognized sometimes at a distance of fifteen or twenty miles from the incessant repetition of these convex forms.[5]

" *The roches moutonnées* owe their peculiar form to the grinding of ice certainly, but they were blocked out anterior to the formation of the glaciers. They were, when the ice quitted them, to what they were before the glaciers began to work, very much like an old worn coin is to one that is newly struck. The hollows were not so much affected, but the eminences were ground down; the depressions in the modelling remained, but the parts in relief were taken away," Whymper urges that before the ice began its work, the surface was very like that of modern weathered rocks. . . . At several places in the Görner glacier, one can get underneath and see the ice bridging over hollows, and it is partially moulded to the surface, as we can see by the flutings upon the bottom of the glacier leading up to the eminences by which they have been caused. When a glacier therefore passes over rough ground, " it is supported upon a number of points and bridges many hollows; the parts of the rock which the ice touches, sustain the entire weight and friction of the glacier and are alone abraded, while the hollows escape. . . . As the ice moves on it strikes the next projection at a lower level than it left the last one, thus the minor asperities of the rock suffer, and chiefly those which are opposed to the direction of the motion of the glacier. They may be actually crushed, or fragments which are already loose may be brushed or scraped away ; in any case they disappear. The glacier, in consequence, becomes supported on a larger area and its power is exerted over a larger surface. It follows that the amount in depth of the matter removed constantly diminishes if the power employed remains the same, while the eminence thus gets abraded. The depressions which are not opposed to the direction of the glacier's motion, if anything like perpendicular to it, remain, and will continue to do so, becoming less and less till the whole bed of the glacier has

[5] " Scrambles in the Alps," 141, 142.

been reduced to a plane surface." Such surfaces are common in Greenland, and Whymper gave them the name of *roches nivelées*. . . . Striations are common upon rocks which are only *moutonnées*, but are rarer or entirely wanting upon *roches nivelées*. They indicate a comparatively early and coarse stage of glacier action ; the water underneath the glacier has more difficulty in discovering outlets, as the rocks become smoother and flatter, and thus assists in reducing the friction and in causing highly polished surfaces by less violent and more uniform abrasion . . . the rotundity of the forms of *roches moutonnées* is a proof that no great amount of destruction has taken place and their *lee-sides* are additional and equally strong evidence. A rough lee-side, unless it can be shown to have been subsequently created, presents us with a surface exposed to the atmosphere before the glacier begun its work, while many lee-sides close together makes it certain that the cubic surface of the bed-rock has been lowered at the most but a few yards. . . . When the beds become flat and smooth, the stones which drop down crevasses have a greater difficulty in getting between the ice and the bed-rock, hence why *roches nivelées* are seldom scratched. . . . As the rocks become smoother and flatter, the friction diminishes and the material removed by abrasion becomes less and less. " Just as a smoothing plane that is set free, will take shaving after shaving from a plank (each shaving being thinner than the last) and at length glides over the wood without producing any effect except a kind of rude polishing ; so a glacier, passing over rocks, takes shaving after shaving in the form of sand and mud, and at length glides on and puts the finishing touches by polishing, to the surfaces which it has formerly prepared by rasping and filing." [6]

Whymper again emphatically disputes the notion that the moraines are a measure of the excavation caused by the glacier, the fact being that the materials of moraines are only in a very slight degree derived from its bed. This is proved by the large glaciers having often smaller moraines than the small ones. As for instance in the two cases of the Görner and the Z'mutt glacier, the former is much larger, while

[6] Id. 142-153.

its moraines are incomparably smaller. The same follows from a comparison of the moraines of the Miage with those of the Glacier d'Argentiere. If moraines were built from matter excavated by glaciers, the moraines of Greenland should be the largest in the world; whereas Greenland is almost without moraines. The same thing follows from the absence of striations, flutings, and groovings over large areas of highly glaciated surface in Greenland. Moraines are not built up of matter excavated by glaciers, but simply illustrate the powers of glaciers for transportation and arrangement. Considerable areas of exposed rocks are essential to the production of large moraines.[7]

Mr. Bonney also objects to the silt which is washed away from the foot of a glacier being treated as a measure of the erosion of its bed. There is often abundance of fine gravel on a glacier which has not come from its bed, and he looks upon the silt as largely " the produce of the grinding of the blocks of stone that fall down the crevasses in that gigantic mill of which the quern is rock, the rubber ice."

Nor does it follow that big glaciers erode more than small ones. " The erosive effect of a glacier depends much more on the grittiness of its under surface than on the weight of the ice. A child with a bit of sandpaper could scour rust away from iron faster than a man with a smooth rubber, and the more the hills are covered, the fewer exposed rocks there will be, and so less rock can fall on the glacier's surface, and so its very bigness will make the file worse. . . . The scouring power of clean ice cannot be great, for Mr. Hopkins determined experimentally that the co-efficient of friction of solid ice upon rather rough sandstone, was tan 20° the same as that of polished marble." [8]

"If I were asked whether the action of glaciers upon rocks should be considered as chiefly destructive or conservative, I should answer without hesitation principally as conservative. It is destructive certainly as to a limited extent; but like a mason who dresses a column that is to be afterwards polished, the glacier removes a small portion

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[7] Whymper " Scrambles in the Alps," 243-247.
[8] Q. J. G. S., xxvii. 322.

of the stone on which it works in order that the rest may be
more effectually preserved." [9]

The real character of the denudation is also described graphi-
cally by Dawson. " Many observers," he says, " have taken for
granted that the mud sent off from glaciers, and which is
so much greater in amount than the matter remaining in
their moraines, must be ground from the bottom of the
glacier valleys, and hence have attributed to these glaciers
great power of cutting out and deepening their valleys; just
as it would be an error to suppose the floor of a grist-mill
ground out of the millstones. Glaciers, it is true, groove
striate and polish the rocks over which they move, and
especially the projecting points and slight elevations in
their beds, but the material which they grind up is princi-
pally derived from the exposed frost-bitten rocks above
them, and the rocky floor under the glacier is merely the
nether millstone against which these loose stones are crushed.
The glaciers, in short, can scarcely be regarded as cutting
agents at all, in so far as the sides and bottoms of their beds
are concerned, and in the valleys which the old glaciers have
abandoned it is evident that the torrents which have suc-
ceeded them have far greater cutting power." [1]

There can be no doubt that this is true. It is the stony
teeth in the ice foot converting it into a kind of Syrian
harrow, which do the eroding, and which, in the process,
get ground down and form the glacier milk which pours
out from the bottom of the glacier. Here, again, how-
ever, we are apt to greatly exaggerate the actual effect.
We are apt to think of the stones below a glacier as held
in a tight vice, and having, therefore, the efficacy of chisels
and engraving tools. As a matter of fact, in consequence of
the viscosity of the ice, they are held in a very gentle grasp
indeed. On this subject we have some curious evidence.

Professor Niles, who in 1878 went a considerable distance
under the Aletch glacier, saw a boulder through a tunnel
of blue ice, which was continued as a deep furrow in the
under surface of the glacier for at least thirty feet from its
beginning. As this was produced by the ice moving over

[9] Id. 153.   [1] Canadian Naturalist, New Series, vi. 413.

and beyond the boulder, it was evident the ice was moving more rapidly than the stone. He says he afterwards saw other instances of the same kind, and argues that stones are not generally frozen into the ice of the glacier, nor held in a fixed position on the under surface of glaciers and carried irresistibly along at the same rate, but that the constantly melting ice actually flows over them, and their motion is one of extreme slowness, even when compared with the motion of the glacier itself.[2]

Mr. Spencer tells us, how, beneath the glaciers of Fondal, Tunsbergdal and Buardal, in the northern, north-central and south-central snowfields of Norway, as well as under other glaciers, he observed many stones enclosed in ice, resting upon the rocks to whose surfaces, sometimes flat, sometimes sloping steeply, they adhered by friction and by the pressure of the superincumbent weight. Although held in the ice on four sides with a force pushing downward, the viscosity of the ice, or the resistance of its molecules in disengaging themselves from each other in order to flow, was less than that of the friction between the loose stones and the rock, consequently the ice flowed around and over the stones, leaving long grooves upon the under surface of the glacier.[3]

At Fondalbrae, an angular stone, whose section was 15,618 inches, rested on the sloping mass of smooth rock. For 20 feet below the stone the under surface of the glacier was grooved by the moulding of the ice about the obstacle. This distance showed the advance of the glacier, after the stone had come in contact with the rock, for it had evidently been completely buried at the lower end of the groove, before the ice had begun to flow about it. As the ice between the stone and the rock gradually disappears, the stone does not suddenly cease to move, but drags, until enough of its surface rests upon the rock to allow of friction between the two granitoid surfaces to overcome the viscosity of the ice, when the latter flows around the obstacle. Elsewhere, the knife-edge of a wedge-shaped piece of gneiss was protruding beneath the ice and resting upon the rock. The front end of the stone

[2] Silliman's Journal, cxvi., p. 366, etc.   [3] Geol. Mag, 1887, p. 168.

had moved beyond the subjacent surface, while the posterior end was still upon it. Yet the sharpness of the edge had hardly been blunted. Abundant examples were found, showing that the flowing of the ice about loose materials was quite the rule. Both large and small (even an inch in length) angular and rounded masses, lying either upon the rock, or upon moraine matter, were sufficient to channel the bottom of an advancing glacier. At Tunsbergdalbrae he saw a stone on an ice slope of 45°, partly embedded in the ice. The ice was clearly moving faster than the stone, which was being rolled and not dragged. The energy upon it had been sufficient to break the stone into two smaller pieces.[4]

Another example of the ability of the ice to flow like a plastic body was shown in a cavern four hundred feet higher than the end of the glacier, where the temperature was 4° C., while that outside was 13° C. Upon the *débris* of the floor rested a rounded boulder, whose large diameter measured thirty inches. A tongue of ice, in size more than a cubic yard, was hanging from the roof and pressing against the stone. In place of pushing the stone along, or flowing around it, the lower layer of ice above the tongue had yielded, and was bent backward, as easily and gracefully as if it had been a thin sheet of lead instead of one of ice a foot thick.[5]

" When there are only occasional fragments in the bottom of the ice, as is commonly the case, the erosion from the sliding ceases as soon as the resistance due to friction between the stones and the rock equals that due to viscosity, which observation shows to be soon reached. . . . Generally speaking, as at Fondal, when the glacier is free from sand, and contains comparatively few stones, the surfaces of the subjacent rocks, although like *roches moutonnées* with angles mostly removed, are not smooth, but are as rough and as much weather-worn as similar rocks in warmer countries, where no glaciers have been ; on their surfaces the scratches are so faint as to be only visible when the rock is moistened. Even the faces of the hummocks are unpolished. In other places, such as Tungsbergdalbræ, which contain much sand along the margin, the rocks are highly polished, and but little scratched. One is every-

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[4] Id. 168-9.   [5] Id. 169.

where surprised to find beneath the glaciers the paucity of glaciated stones, and in many terminal moraines they are scarcely, if at all, to be found." [6]

The insufficiency of glaciers to act as great erosive agents is further shown at Fondalen, where a mass of ice, thirty or forty feet thick, abuts against a somewhat steep ridge of rock, ten feet or less in height. In place of a stone-shod glacier, sliding up and over the barrier, the lower part of the ice appears stationary, or else is moving around the barrier, while the upper strata bend and flow over the lower layers of ice. [7]

" At the Supheliebræ the glacier was moving up the slight elevation or moraine produced by the early summer retreat of the ice. The lower surfaces of the ice tongues were furrowed by the loose stones of the soft, incoherent, water-soaked moraine, into which one's foot would sink when stepping upon it." [8]

This is, assuredly, very interesting evidence to show how slight the eroding action of ice is, even when armed with serried teeth of stone and pressing with a heavy weight on its bed. Let us now come to closer quarters with the theories we are criticizing.

The first person, I believe, who suggested that ice could act as an excavator was Dana, who put out this view in giving an explanation of fiords in the tenth volume of the U. S. Exploring Expedition, published in 1849.

In the year 1862, two papers appeared in the Philosophical Magazine, both of them the works of distinguished men, Sir A. Ramsay and Professor Tyndall, the question raised by the former writer was whether glaciers had excavated the greater number of mountain lakes, a question which he answered in the affirmative. Professor Tyndall went much further, and argued that the contour of mountain regions and the excavation of mountain valleys, was entirely the work of glaciers. Two schools of geologists entered the lists on this important discussion—those who advocated and those who denied the excavating power of ice. Among the former may be numbered Ramsay, Tyndall, Geikie, Helland, Stark, Logan, Haast, Jukes, Newberry, Ward, K. Steenstrup, and Penck.

[6] Id. 170.   [7] Id.   [8] Id. 171.

Among the latter Agassiz, the Duke of Argyll, Bonney, Credner, Desor, Escher von des Linth, Falsan and Chantre, Falconer, Favre, Gurlt, Heim, Hoernes, Kjerulf, Lyell, Martins, Mojsisovics, Murchison, Oldham, Onitsin, Reclus, E. Richter, Rutimeyer, Stoppani, Whitney, Zöppritzch.

I cannot, of course, profess to give a full account of the controversy, and can only condense that portion of it which seems to me to be conclusive.

In limine it is a curious fact that the arguments of Tyndall and of Ramsay are mutually destructive.

In the *Phil. Mag.* for October, 1864, Tyndall urged that the cutting power of a glacier gradually relaxes as we descend, and finally, when the eroding agent quits the mountains altogether, the grand effects which are produced in the earlier portions of its course entirely disappear.[9]

Ramsay's case, on the other hand, requires that glaciers should maintain their erosive tendencies long after they leave their mountain cradles. Only in this way could they have excavated the lakes which occur so frequently in long flat valleys. His argument is, that "when a big glacier reaches the plain its erosion would cease but for the pressure from behind. This pressure, combined with the vertical pressure, results in its direct erosive power being necessarily proportioned to its thickness, and greater than when it lies on a slope, for it grates across the rocks, as it were, unwillingly, and by compulsion, instead of pushing its way onwards by means of gravity only." The sluggish ice has a tendency to heap itself up just outside the mouth of the valley, and there attain an unusual thickness, thus exercising after its descent an extra erosive power.[1] It will thus be seen, that, however their results appear to agree, the arguments of Ramsay and of Tyndall are mutually destructive. Let us pass on, however. Both of them appealed to the erosive tendencies of ice, and both take it for granted that the ice can and does excavate the ground like a plough-share, utterly ignoring facts and arguments such as I have just adduced. Let us first examine Tyndall's position.

His famous paper was entitled " The Conformation of the

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[9] Id. 214.　　　　　　　　[1] Phil. Mag. 1865, 287.

Alps." He urges that there are insuperable difficulties to the notion, that the present mountains have arisen through the action of forces localized beneath their bases, or that the valleys, as they now exist, have sunk through want of local support underneath. *A general elevation* of the land must be assured, and the question then occurs, how has the land thus elevated been carved into its present form ? He argues that the denuding power of water is very slight. Speaking of the valley of Hasli, he says, " A million winters may have acted upon these scarred and fluted rocks, and still the scars and flutings are as distinct as if they had been executed last year. We trace them down to the banks of the Aar, a river which has been rushing for these ages through the valley, and the smoothness of its operation must impress us with the *comparative* feebleness of denudation by water. The same is the case with the polishing, etc., in the Rhone valley." [2] Speaking of other valleys north of the Alps, he says, " What air and water have accomplished since the disappearance of the glaciers, are mere scratches of the tooth of time, in comparison with the mighty furrows which had been previously ploughed out." He concludes " that all this mountain region was held by ice, enormous as to mass and in incessant motion. That such an agent was competent to plough out the Alpine valleys cannot, I think, be doubted. . . . That the glaciers were the real excavators, seems to me far more probable than the supposition that they merely filled valleys which had been previously formed by water denudation. . . . This view will carry us still further. According to it a glacier is self-destructive. The more deeply it ploughs the surface of the earth, the more must it retreat . . . everyone of these valleys is a kind of furnace which sends draughts of hot air up to the heights, and thus effectually prevents the formation of ice. . . . Elevations of the land, which would naturally accompany the gradual cooling of the earth, are quite competent to account for a glacial epoch, and the ice itself, in the absence of any other agency, would be competent to destroy the conditions which gave it birth." [3] I am not aware that Professor Tyndall has ever withdrawn these views.

[2] Phil. Mag. 4th series, xxiv. 171.   [3] Id. 172, 173.

They were immediately traversed, and by no one more directly than by Professors Ramsay and Jukes, both of them believers in ice as an excavator. Ramsay had already committed himself to a very different view, and had affirmed that previous to the tertiary glacial epoch, most of the grander contours of hill and valley were in Britain and elsewhere in Europe and America nearly the same as now.[4] Writing again, in 1862, speaking of the Alpine valleys, he says : "No true geologist is likely to assert that these valleys have been mainly scooped out from end to end by ice, merely because running water has, since the disappearance of the ice, effected so little. He urges that this is because time has been too short for it to have done much, and he argues that the eroding influences of water had been at work long before the glacial period, and far back into tertiary times. . . . The larger valleys were in their main features approximately as deep as now before they were filled with ice." He quotes, in proof of this, "the great moraine of Ivrea, 1600 feet deep, proving that when the glacier issued from the valley upon the plain, the ice at the sides was at least of this thickness and perhaps thicker in the middle. At the mouths of the valleys debouching on the valley of the Po, there are ancient alluvial fan-shaped masses of gravel, quite like those left by existing torrents. These were deposited when the true mountain valleys, at all events near their mouths, were just about as deep as they are now, for the great glaciers overflowed their gravels and deposited their moraines upon them, only in part scooping them away. No better proof," he says, "could be required, that in great part the valleys of the Alps were approximately as deep before the glacial epoch as they are at present; and I believe, with the Italian geologists, that all the glaciers as a whole effected was only slightly to deepen their valleys and materially to modify them and further to deepen them in parts more considerably where, from various causes, the grinding power of the ice was unusually powerful."[5]

In February, 1863, Mr. J. Ball wrote a paper in the same magazine. In this, he points out " that the chain of the Jura

[4] The Old Glaciers of Wales, 94.   [5] Phil. Mag. 1862, 380.

nowhere consists of a single ridge, but of three, four or five parallel ridges, with furrow-like valleys lying between them, here and there cut through by some stream that appears to flow through a fracture that has traversed the entire range," and he says, very reasonably, "that this system of valleys cannot be explained by Tyndall's theory, for, supposing the glaciers competent to form valleys, they would shape them at right angles to the range." In regard to the main chain of the Alps, he points out "that the great valley of the Rhone virtually cuts it in two. A parallel valley more or less broken is that of the Inn. The same direction prevails among the valleys of the central region, and these lines of depression traverse ridges or contain streams that flow in opposite directions—showing that by no conceivable change in the general configuration of the land could a single stream or glacier have done the work . . . when taken together, the facts bring conviction to my mind, that some considerable portion, at least, of the existing valleys in the Alps owe their origin to forces which have operated on a great scale, and which can scarcely be other than those which have raised the mountain ridges to which the same valleys are related."

"If we examine particular groups or masses, we find," says Mr. Ball, "indications of a common direction in the secondary ridges and valleys, transverse, but seldom exactly perpendicular, to that of the main valleys . . . all showing a degree of parallelism which points to the operation of some common cause," and he quotes as examples *inter alia* " the seven minor valleys lying east and west between the Val Magia, and that of the Jura, where there is nothing in the configuration of the district to make it conceivable that, if the hollows were filled up with water or ice, they would re-open trenches where the present valleys exist."[6]

Mr. Ball further argues that "a glacier slides over concavities without touching them, and applies all its grinding power to the convex portions of its bed, and that consequently valleys excavated by glaciers should show one uniform model. Instead of this, many of the Alpine valleys consist of a series of level basins rising in steps as we mount them, and connected by gorges which are always much narrower than the basins

[6] Phil. Mag. xxv. 81-84.

they link together. Even if it were admitted that glaciers can excavate lakes, the existence of a succession of approximately level steps, separated by steep slopes and narrow gorges, is conclusive proof that some agency, other than that of ice, must have directed the original formation of valleys of this type. In one respect the inequalities of the sides of Alpine valleys offer a stronger argument against their glacial origin than those of their bed . . . it is inconceivable that a glacier should be competent to scoop out valleys a mile or more in depth, and yet be unable to remove the main inequalities from its own channel."

"During the long period the Mer de Glace must have been flowing, it has not succeeded in grinding down the ledge over which it flows, and the ice-fall still remains. . . . The same argument might be applied to lateral obstacles, such as the rocks known as L'Angle on the west side of the same glacier."

In regard to Tyndall's views, as to the general contour having been determined by ice, Bell points out how the main chains of the Pennine Alps are serrated with peaks and intervening depressions, and that glaciers, no matter how extensive, or endowed with any possible mechanical power, would not have determined the form of the topmost crest of that great range, or reduced it below its present level. The grinding power of ice is effective only when it flows in defined channels, and the snow and ice on the higher Alps protect them from destruction and do not denude them.

Lastly, Mr. Ball calls attention to the fact that the limit between the zone where the rocks have been smoothed and striated by the old glaciers, and the part which they never reached, is as easily followed as is the line of high-water mark by the fringes of seaweed on the shore, "and if this mark avails as positive evidence of the former extension of glacier action, it is not less a negative proof that that action did not surpass certain assignable limits."[7] This evidence, in fact, shows how very slight a skin has been scored off the rough rocky surface by the ice.

We will now turn to Mr. Whymper's arguments, which seem to me effective and clear. He tells us how in the valley of Aosta, while the *roches moutonnées*, with their polished and

[7] Id. 84-88.

striated surfaces, remain almost unchanged since the glaciers moulded them . . . numerous angular rock surfaces occur alongside of them, having their lee-sides all rough, showing that ice never touched them. These rough surfaces cannot be due to the subsequent effects of snow, frost and water, which have not altered the polished faces, nor cut away the striæ and marks lower down.

The existence of these unworn surfaces shows that the valleys were not excavated by the glaciers, while the comparative scarcity of *roches nivellées* combined with other evidence, proves that the so-called excavation has not amounted throughout the valley to more than a few feet.

If the valleys had been excavated by ice, the surface of the rocks would have been as smooth as glass from one end to the other. Why should its effects have been effaced at heights above 9000 feet, and preserved lower down? Above 9000 feet the mountains are rugged and angular. Agassiz said long ago, " above the line at which indications of ice action cease, the edges of the rock are sharp and angular, the surface of the mountain rough, unpolished, and absolutely devoid of all those marks resulting from glacial action. On the Alps these traces are visible to a height of 9000 feet." [s]

The entrances to the valleys again are usually much wider, and their beds at a lower level than higher up. This is unintelligible if the valleys were excavated by glaciers flowing down them, which lessened in eroding power as we descend.

How again did such obstinate crags as that at Montalto at the entrance of the Aosta valley, and those on which the walls of Sion stand, remain intact in the very centres of the valleys? They were exposed to the full force of the glaciers and, in fact, must have split them in two. If this was due to the adjacent rocks having been softer, how is it that their surfaces are flat, and less abraded than those of the crags? To all appearances these surfaces have not been lowered more than a few feet. The adjacent rocks have, in fact, been less abraded than the crags.

On the same subject Mr. Bonney says : " If glaciers made the valleys, their main contours should lend themselves readily

[s] Atlantic Monthly, Feb. 1864.

to this explanation, while, as a rule, the upper part of the Alpine valleys, where the glacier action may be supposed to have been the most recent, and therefore most affected by other denuding agents, we are struck by the steepness of the last few miles up to the watershed as compared with the inclination of the rest of the bed." He refers in detail to several instances proving this. " If," he continues, " it be conceded that the névé of a glacier can settle down so fast that the uppermost mile or two of its bed shall be almost a cliff compared with the remainder of the stream, and we thus account for the configuration of many valleys, . . . how are we to explain such contradictions as the Mont de Lans, the Théodule, the Görner, and the Titlis glaciers? or such anomalies as the low opening of the Ampezzo Pass (about 5000 feet) among the towering summits of the Drei Zinnen, Cristallo, and Geiselstein? Surely it is far safer to suppose that the glen, by whatever cause fashioned, gave rise to the glacier, rather than the glacier to the glen."

Referring to the Unter Grindelwald glacier, which had retired 500 feet between 1850 and 1870, he says: "It can now be seen that the final ice fall descends over three or four steps of hard limestone; the last and perhaps the tallest of them, together with what we might call the first flag of the valley floor, being at present exposed. If then the glacier has made these steps, if it be plastic enough to mould itself to them, which it is not in all cases, surely it ought, at the base of each wall, to have worn out considerable hollows, analogous to the pot holes at the foot of a cascade. But there is nothing of the kind to be seen, the rocks, beautifully polished and scratched in many places, exhibit the usual contours, and after forming an irregular lumpy terrace showing often " Floss and Lee seite," round away to the next cliff." [9] The steps not uncommon in valleys such as those in the Val Formazza, and that which is visible from near Landro, in the glen which descends from under the Drei Zinnen to the Dürren Sea, present similar, but still greater difficulties in accepting the hypothesis of the glacier erosion of valleys.

He next refers to such valleys as the Pusterthal, a great trough whose sides are steep mountains, rising 4000 feet or

[9] Quart. Journ. xxvii. 319-320.

more on either hand, while its base dips gently from a central plateau to opposite points of the compass, and he urges that no one who has seen it can conceive it to have been hollowed out by a glacier.

Mr. Bonney adds : " I have shown [1] that in several districts of the Alps there is evidence that glaciers have descended important valleys, yet have been incompetent to modify their principal features. This argument I venture to assert has never been met. Every year I travel gives me fresh instances." [2]

It is a remarkable fact that those people who claim the greatest possible results from the eroding powers of ice are also sturdy advocates of ice sheets traversing the whole country irrespective of obstacles and in directions contrary to its drainage, and this when its power must have been at its maximum. On this subject the Duke of Argyll has written well. He says : " If our existing systems of hill and valley are due to glacial action, that action must have been coincident with the existing lines of drainage, and no action can be quoted as proof of this effect which has been traverse to those lines. Valleys cannot have been cut out by an agency which has not worked along them, but cut across them. The contours of mountains cannot have been determined by any force which has worked irrespective of the valleys in which and out of which those mountains stand. And yet we hear constantly of ice-marks which are totally independent of the existing drainage, marks which indicate the passage of masses not coming from any existing summit and not travelling down any existing hollow. These masses are supposed to have moved up and down dale, in some definite direction at every variety of angle with the existing lines of mountain and valley. . . . I venture to submit that these valleys cannot have been cut out by masses of ice which, so far as they cut at all, must have cut in a different direction." [3]

If, as is conceded, the ice-sheet when working at its greatest height did not carve out channels for itself, but only scored the

[1] Q. J. G. S. xxvii. 312, xxix. 382, xxx. 479.
[2] Geol. Mag., New Series, 1875, 428.
[3] Q. J. G. S. xxix., lvii.

surface athwart its lines of drainage for many miles, two things seem to follow. First, that when it had shrunk, as it is supposed to have shrunk, into smaller glaciers, it could hardly have done so. Secondly, the very striæ themselves going over the uneven surface show that the valleys and hills were there just as they are now before the ice-sheet moved over them, and must have remained so afterwards, or their chisel marks would have disappeared. Not only so, but if we are to treat the question empirically and test it by existing glaciers, we must concede with the Duke of Argyll, " that the pre-existence of valleys and hollows for the accumulation of the ice necessary to the formation of a glacier seems as necessary as the pre-existence of the summits which condense and collect the snows."

He further points out that Mr. Croll's discovery of ancient river channels buried beneath drift proves that the great depression which connects the valleys of the Clyde and the Forth, along the line of the Clyde and Forth Canal, had the same direction it has now in pre-glacial times, and was occupied by streams showing the same system of drainage.

Mr. J. C. Russell, himself a great champion of glacial erosion, says: " Those who attribute the formation of the Yosemite and other similar valleys to glacial erosion, should be required to point out the moraines deposited by the ice streams that are supposed to have done the work. The glaciers of this region were so recent, that all the coarse *débris* resulting from their action yet remains in the position in which it was left when the ice melted, and if the magnificent valleys referred to are the result of glacial erosion, moraines of great magnitude should be found about their lower extremities. Observation has shown that *débris* piles of the magnitude and character required by the hypothesis are notably absent, and he concludes that the excavation of many of the valleys of the Sierra Nevada began long previous to the quaternary, and are relics of a drainage system which antedates the existence of the Sierra as a prominent mountain-range. He further adopts the hypothesis of Mr. J. D. Whitney, that the main characteristics of the valleys are due to dislocation, or, in other words, that the orographic block beneath the valley has subsided.[4]

[4] See Quart. Hist. of Mono Valley, California, 350-354.

While the appeal to ice as the excavator of valleys seems to fail wherever tested, there is another explanation which seems to be complete, and to meet the whole case. We are too much accustomed to look upon valleys as having smooth contours and curved outlines. This is because we only see the soft mantles of gravel, sand, and clay with which they are lined, and which have been thus smoothed down by having been laid down in continuous sheets. If we could strip our valleys of these coverings, we should be greatly struck and greatly startled by the rough, broken, and dislocated contours of the subjacent rocks, the result of very different forces to that by which the soft beds have been spread out. "The Rossendale Anticlinal on one side of the Pennine chain and the Craven Faults on the other," says the Duke of Argyll, "are the keys to the great valleys of Lancashire. The Ribble runs along an anticlinal in the carboniferous strata. These are significant indications. They imply that the coincidence which is wanting between the features of the country and the lines of glacial denudation, is a well marked connexion between these features and the lines of subterranean disturbance."

I do not propose to carry this analysis further, for I do not know any geologist who now maintains seriously the view that the valleys were the result of glacier erosion. It is different, however, with those curious hollows generally called cirques, and which were first so called, I believe, by J. de Charpentier in his "Memoir on the Pyrenees," published in 1823, page 24. The Pyrenean peasants call them *oules*, i.e. cauldrons. They are known in our own country by other names, such as corries and combes or cwms, and in Scandinavia as *botner*, (i.e. bottoms) or *kare*.

Mr. Russell and Dr. Wright define these hollows as deep semicircular excavations, bounded on all sides, except that through which the drainage escapes, by high cliffs or by perpendicular walls from a few hundreds to more than a thousand feet in height. The bottoms of these excavations are often depressed below that portion of the river through which the drainage escapes, and form rock basins; at other times the rock basins are partially enclosed by *débris*, and in some instances they have well-formed terminal moraines across their

outlets.　In these hollows there are transparent lakes of azure blue . . . a horizontal section of a *cirque* is semicircular or horse-shoe shaped, and in certain portions of the range they are so numerous that they give a scalloped contour to the faces of the cliffs.　The interiors of some of the amphitheatres are terraced. . . . In places the cirques occur on either side of a fragment of table-land, and have been eroded back until only a knife edge of rock, so narrow and broken that the boldest mountain climber would hesitate to traverse it, is all that divides one profound depression from another. . . . They were the fountains which gave birth to the ancient glaciers, and were also the last strongholds to be abandoned when the reign of ice approached an end.[5]

Mr. Bonney says he " can draw no hard and fast line between cirques, corries, and even the ordinary bowl-shaped heads of valleys so common in any country which has been subjected to meteoric action.　The small passes into the large, the slope steepens into the precipice. . . . I have even seen cirques on clayey banks in miniature, with walls that might almost be measured by inches.　Connecting links may be found, through corrie and cwm, between the merest bowl-like hollow on some down of chalk or oolite, or even some plateau of sandy clay and the grandest cirque of the Alps or Pyrenees.　The differences are due to the nature of the materials and to other local circumstances." [6]

He says aptly, " that whatever the origin of the cirques, whether caused by upheaving forces, or denudation by waves, currents and rivers, all of which may have prepared the surface for the glacier, the glacier would efface the tool marks of these preceding agencies, just as when speaking of a statue as carved by a chisel, we take no note of the crowbars, wedges, blast powder and other appliances which were employed to quarry the block before it was placed in the sculptor's hands." [7]

If the cirques were formed by glaciers, it was either by glaciers which took their rise in them, or, like the concavities in the course of a river, by the action of a passing ice-stream.　In regard to the first issue, he says, " since the ice at these

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[5] Wright, Ice Age in America, 242-244.
[6] Geol. Mag., new series, iv. 275.　　[7] Quart. Journ. xxvii. 315.

heights would be in the form of névé the erosive action would be very slight, the friction being due solely to the weight of the superincumbent ice, and as there are comparatively few stones in the névé, there would be few grooves. I can conceive it possible that if a glacier, after wearing away a stratum of hard rock, reached one in which erosion proceeded more rapidly, it might deepen its angle of descent to some degree in the upper stratum also, but I cannot conceive that precipices more than 1000 feet high could be thus produced, and we must remember that in the most conspicuous of the cirques described the most compact rock forms the lowest stage. . . . We may venture to assert that it is mechanically impossible for a glacier to sink into its bed so as to excavate a cirque on so gigantic a scale."

Secondly, if, as has been argued, cirques were produced at some bend in the ice-stream like the concavities in a river's course, the Creux de Champs and " Am Ende der Welt " could not have been so, because they are at the head of deep, well-marked, lateral glens about a mile and a half long, while in regard to the two cirques on the Blacken Alp, the spurs from the Blackenstock and Rothschutz would effectually shelter them from the action of any glacier descending from the Surenen Pass (7562 feet), but apart from this, existing glaciers do not appear to produce such effects in turning corners in their present beds."[8]

Speaking of some of these hollows in Skye, he says : " These cirques have been occupied by glaciers, and the ice-worn slopes at the very foot of the cliffs show that comparatively little change has taken place since the time when the last snow melted away from the head of the glen. They therefore were prior to the glacier, and far more the cause of it than it of them. There must have been cirques when the approach of a polar climate first enabled the winter snow to remain in the sheltered nooks all through the summer. . . . In crystalline, as in sedimentary districts, the action of glaciers was little more than superficial.[9] To suppose that a glacier could have made them, is to endow it with a property of vertical excavation that I think the most ardent glacialist would hardly claim for it, and the comparatively small elevation of the mountain crest behind them

[8] Quart. Journ. Geol. Soc. xxvii. 316, 317.   [9] Id. 539.

showed that they could not be explained by any hypothesis of a long slope of snow pressing downwards upon the ground beneath it. The only long slope is that of Ben na Caillich, and that not only shows no corrie, but is in the wrong position." [1]

Mr. Bonney was replied to by Gastaldi in 1873. He describes a cirque as having the shape of the interior of an armchair, or more elongated like a sofa. The back of the armchair is formed of scarped walls cut in the thickness of the mass of the mountain ; the two sides by ridges which descend from the mountain, and the seat by the inclined slope of the mountain, the surface of which is *moutonnée,* often striated, and here and there covered with the *débris* of moraines. He then goes on to describe the cirques as occurring in hard rocks as well as in soft ones, and, without explaining how they were made, merely concludes that the high cirques are the beds formerly occupied by glaciers, adding, " Glaciers, therefore, are well able to excavate for themselves deep beds in soft rocks, and also in rocks relatively hard, in high alpine regions." The Duke of Argyll said very aptly, in answer to this, " that the fact of cirques having once been filled by ice does not imply that they had been made by the ice, and that the precipices surrounding the cirques could not have been cut down by ice." [2]

Mr. J. C. Russell, while differing from Mr. Bonney in his explanation of the way in which cirques were formed, agrees that their general outline was given them by water, but that for the final sculpturing, and in order to form typical cirques, another tool was required ; that tool was ice. " The prototypes of cirques he finds in the depressions about the summits of the Southern Appalachians, which were never subjected to glacial conditions. Again," he says, " cirques are confined to mountains on which ice sculpture followed water sculpture. The topographic forms left after the disappearance of the ice are modifications of the antecedent forms due to the action of rain and streams. . . . Glaciers were formed on unmodified slopes, but have not excavated *cirques* for themselves." [3] The difference between the two writers is largely one of detail. I cannot, however, disguise my strong preference for the views maintained by Bonney.

[1] Id. 537 and 8.   [2] Q. J. G. S. xxvii. 396-401.   [3] Nature, xlv. 318, 319.

In regard to the ice erosion of cirques, Mr. Goodchild, in 1875, urges that as a glacier scoops only in a downward and outward direction, all traces of horizontal projections should have been ground off by the ice in making the cwm, and instead of terraces at right angles to the path of the ice we ought to find furrows. Yet where the rocks forming the cwm consist of a nearly horizontal set of alternations of beds of different kinds of hardness, each bed forms an amphitheatre shelf or terrace, which is separated from those above and below it by a horizontal interval, often of considerable extent, so that the general effect resembles on a gigantic scale the tiers of seats in a vast amphitheatre. This is notable in the case of the Jura. It is clearly impossible that these ledges can be due to the erosive action of ice, and he agrees with Bonney that simple glaciers have had little or nothing to do with the formation of the greater number of cwms.[4] If the cwms were excavated by glaciers, the greater erosive force must have been exerted in the lower parts and not the higher, where the névé is hardly ice at all. Yet the lower parts are very like ordinary valley heads, and it is the upper parts that exhibit the greatest erosive effects. The glacier ought to undermine and not to leave steps if its work was done where its chief force lay. Lastly, cwms occur so commonly near the snow-shedding line that the glaciers could not have exerted much pressure on their beds there, thence it seems impossible that an ordinary glacier, or indeed ice in any form, moving in the way a glacier does, could give rise to the crater-like recesses whose origin is being discussed.[5]

In 1876, Helland read a well-known memoir before the Geological Society, taking the same view as Gastaldi. Cirques are called "Botner" (bottoms) by the Norwegians, and he describes one in Greenland, between the Valley of Upernivik and the mainland, as enclosed by vertical walls nearly 1000 metres high, and says the two walls of a cirque often approach one another, so that a horizontal section is more than a semicircle, adding that they chiefly face the north in Norway. He argues " that they are found where glaciers either occur or have occurred, and at different heights varying to some extent

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[4] Geol. Mag. New Series, ii. 490.
[5] Id. 490, 491.

with the snow-line. They are situated chiefly near the snow-line, and he associates them with the formation of isolated glaciers which also prevail at the same level ; and adopts the view of the engineer Lorange, that the temperature around the glaciers constantly varies about the freezing point, the incessant freezing and thawing splits the rocks, and the ice then abstracts and transports the fragments, and says " that on examining the interior of an empty cirque we observe that a bursting, not a scooping out, of the rocks has taken place." [6]

In a paper in the *Geological Magazine* for 1877, Mr. Bonney urges that the reason isolated glaciers abound where cirques abound, is because a cirque with its sheltered recesses is peculiarly favourable to the formation of a glacier, and proves no more than the existence of a puddle in the sheltered corner of a quarry proves that the water excavated it. Helland's theory involves a kind of perpetual tooth-drawing; the bed or slope of the mountain beneath the glacier is crushed by changes of temperature, the fragments are caught by the superjacent ice, lifted up, perhaps wrenched out and borne away. Now the beds of the rocks in the cirques are smooth, while they should be pitted with the sockets of extracted teeth ; the sharp-edged blocks he refers to have either fallen directly from above or been dropped by the melting glacier. According to Helland the tarns in the cirques were not scooped out like big lakes, for one often sees sharp-edged blocks covering the bottom, and he considers that their broken bottoms are due to the lakes having been filled with ice which has broken the rocks. " If so," says Bonney, "how were the excavated materials removed from the deeper tarns ? "

The walls of the cirques in the Alps and Pyrenees are often 2000 ft. or 3000 ft. high. Those of the Creux de Champs are twice that height, rising with inaccessible steepness, interrupted by mere ledges from the floor of the cirque to the crest of the Diablerets. " Does Mr. Helland," says Mr. Bonney, " seriously mean that ' a small isolated glacier ' has settled almost vertically downwards on the site of the cirque, deepening its bed by 1000 yards, and that too beneath its névé, where the erosive power is weakest ? We should want the rotatory glacier whirlpools of ecstatic ice like whirling dervishes, which

[6] Q. J. G. S. xxxiii. 161-165.

Mr. Ruskin long ago suggested, to perform work like this.
. . . If this energetic glacier was limited to the immediate
vicinity of the cirque, then the floor of this part of the valley
ought to be lower than that further down; in other words,
there ought to be a deep lake beneath the walls of the cirque,
which there is not; or else the valley must have been un-
commonly deepened and modified, almost excavated by the
glacier. . . . It is exactly this which is not only unsupported,
but even opposed by the evidence of the Alps and of every
mountain region I have seen. . . . In the valleys we have
little or no indications of any but the most superficial works
of glacial erosion. . . . If then the erosive action of glaciers
is so slight that it is difficult to credit them with the greater
Alpine lakes, how can we attribute to them cirques which
occur just at the point where they are feeblest, and where the
connection with the formation of the whole valley is most
inseparable?"[7]

Elsewhere, Mr. Bonney protests against the notion that the
base of the névé can act as a tooth-drawing machine, and thus
disintegrate the glacier bed. "Wherever," he says, "I have
seen the bottom of a corrie uncovered, the rock has been
smooth and ice worn, not rough with the sockets of extracted
'plugs' of rock." He further argues that the atmospheric cold
is not appreciably more potent at the base of the névé than at
the bed of a glacier, which Helland's and Russell's views require.
Again, since the bergschrund usually narrows in descending,
it seems clear that the tension of the ice is at its minimum
at the base of the névé. Again, as the latter stands to the
glacier in the position of a pond to its affluent, and its motion
is much less, so must the friction be less. The stupendous
nature of the tooth-drawing process is another difficulty.
Above some of the cirques the cliffs rise in inaccessible steeps
to the very summit of the range. From the highest peak of
the Diablerets one could almost throw a stone on the floor of
the Creux de Champs, a full mile below, the two cirques near
the Surenen Pass are immediately under the crest of the range.
Lastly, some of the Alpine cirques are now fully 3000 feet
below the snow-line and are not in a position where glaciers
would first appear. Mr. Bonney add that it is quite con-

[7] Geol. Mag. New Series, iv. 273-276.

trary to all he knows of the Alps and other mountains to suppose rock wrenched away to a depth of a thousand yards and wrenched away by masses of névé, whose thrust must be equal in all directions, in a way so as to create great precipices and perpendicular steep walls of rock.[8]

Mr. J. Geikie confesses that he cannot see his way to account for the vertical cliffs surrounding the cirques by invoking ice action, while Mr. Jukes Brown refers to a cirque he saw in Egypt where ice action is impossible, and which he calls a rain gorge.[9]

Let us now turn to another part of our subject, namely the origin of fiords.

The first person, I believe, who suggested that ice could act as an excavator was Dana, who, writing in 1849 of the origin of fiords, points out that they are frequent in the higher latitudes and almost totally absent from the lower temperate and torrid zones. They abound, he urges, on the west coast of America, north of 48° and to the south in Lower Patagonia and Tierra del Fuego south of 48°, but in between, the coast has few bays and fewer still of these channel-like indentations. On the east coast it is the same, until we reach the north of Maine, in lat. 43°; going north we do not find them, then the fiords become very numerous and deep and complex in their long windings and ramifications. The same is the case with the Eastern Continent. The fiords of Norway are well known, and its coast is in singular contrast with those of France, Spain and Africa. He then goes on to argue that the fiords must have been formed in general either by *marine denudation*, by *subaerial* denudation, or by rupturings of the earth's crust, and he concludes that the fiords occupy old valleys of denudation, which have become fiords by submergence, and suggests that they may have been executed more or less by glaciers.[1]

This view was dormant for many years, and it was not till 1862 that Professor Ramsay revived the notion in a paper on "Glacier Erosion," already quoted. He argued that some of the Scotch lochs owe their depth to the grinding power of the ice that of old flowed down the valleys when the land was higher

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[8] See *Nature*, xlv. 391.   [9] *Geol. Mag.* 1877, 477-479.
[1] Report U. S. Expedition, x. 675-678.

than now, and suggests that both lochs and fiords may have been the result of ice action. Sir A. Geikie in his "Scenery of Scotland," pp. 127, 183, etc., makes a similar suggestion.

In 1869 and 1871, Mr. Brown read papers on "Fiord Excavation" before the Geographical Society. He tells us that after a long and careful study of fiords in most parts of the world, he had come to the conclusion that we must look upon glaciers as the material which hollowed them in such a uniform manner. "Everywhere," he says, "you see marks on the sides of the British Columbian fiords of ice action . . . which, by long abrasion, hollowed out the valleys into the forms we now see in the fiords." He admits, however, that previous to the existence of the fiords there were valleys and depressions, and even ravines and gulches, formed by volcanic action which were afterwards fashioned into fiords, but he is emphatic that fiords, as we understand them now, did not exist before the ice began its work. He seems to explain the *modus operandi* as first the converting of a valley into a deep glen, when by the aid of the oscillation of the coast, the sea would enter it and carry off the glacier as icebergs, its former bed having become the fiord through which the icebergs could sail.

Mr. Murphy, in a paper read before the Geological Society, urges that nearly all coasts that are cut up by fiords are western coasts in high latitudes, and after quoting some examples, he adds, " The transition from straight to indented coast lines coincides pretty equally with that from dry to moist climates. . . . It is now generally believed that the prevalence of lakes in high latitudes is in some way a result of glacial action ; *it can scarcely* be doubted that this is equally true of fiords, and the coasts I have mentioned are those on which glacial action must necessarily be most energetic, because west coasts in high latitudes are exposed to west winds, Maury's ' counter trades,' which deposit on the mountains in snow the moisture they have taken up from the sea." [2] This last sentence ignores the fact that the anti-trades in the eastern hemisphere blow in the reverse way, and the Chilian fiords ought therefore to be on the other coast. The fiords existing on the Asiatic side of the Bering Straits always seem to me to be a great difficulty in the way of those who claim fiords as

the result of ice erosion. We have no evidence that glacial conditions ever prevailed in this district. The evidence is very much the reverse, for unless a mild climate had existed there, the pleistocene mammals could not have passed to and fro between the Old World and the New.

Murchison, in his address to the Geographical Society for 1870, says, "That these enormously deep and broad cavities have been excavated entirely by ice, as Mr. R. Brown argues, is a hypothesis which rests on no sort of evidence. Where in any icy track is there the evidence that any glacier has by its advance excavated a single foot of solid rock? In their advance, glaciers striate and polish, but never excavate rocks." Speaking against the notion that ice excavated the fiords and water the cañons, he continues, "The plain and unmistakable geological conclusion, is that wherever the earth's crust was broken up from beneath, it necessarily underwent great transverse cracks, which opened into fissures and caverns, and these openings made at different times were then left to be operated upon in subsequent ages by all the waters which fell on their surface, or by rivers above and below that surface, to be by them abraded and fashioned. The true origin of all such great transverse fiords, or cañons, or, in short, of all abrupt fissures in hard rocks into which bays of the sea enter, or in which rivers flow, must be referred to original breaks in the crust, of which the waters have taken advantage, and have found the most natural issue."

Mr. Brown admits that the channel of the Golden Gate at San Francisco has a maximum depth of fifty fathoms, and that this great chasm is in the line of the axes of the elevation of the main chain, which is a clear evidence that it was due to subterranean agencies, and not to the diurnal action of ice and water, which have only modified the old breaks and cavities.[3]

In a paper on "Greenland Fiords and Glaciers," Mr. J. W. Taylor, who had spent eighteen years in Greenland in connection with the cryolite mines of Arksut Fiord, says that he had never seen anything to lead to the conclusion that the fiords were hollowed out by glaciers. He maintains that the reverse is the case, that instead of glaciers excavating fiords,

[3] Journ. Roy. Geog. Soc. xl. p. clxxiii. and clxxiv.

they are continually filling them up. " It is true that boulders
and *debris* borne along by the ice, scratch, polish and grind
the rocks to a considerable extent; but though strong as a
transporting agent, ice alone has little excavating power; it
is like the soft wheel of the lapidary, the hard matter it
carries with it does the polishing."

The fiords of Greenland are walled in by rocks averaging
1000 feet in height, their length varies from 10 to 100 miles,
their breadth 1 to 8 miles, and the depth of water from a few
feet to 200 or more fathoms. The rocks on each side of these
fiords are marked by ice action at intervals, but more so
near the glacier.

Some of the largest glaciers do not exist in fiords at all;
thus the glacier north of Frederichshaab is fifteen miles broad.
It has made no fiord and launches no icebergs, and for this
reason it has brought down a lot of loose material to a reefy
coast, and formed a beach at its base, and this great ice power
which we are asked to believe has excavated fiords in granitic
rocks, 100 miles long and 3000 or 4000 feet in depth, is over-
come by loose *débris* and sand. Why does it not cut its way
through these, by far the easier task?

There are numerous fiords in Greenland nearly filled up by
loose material brought into them by glaciers; the glacier
becomes as a power (if I may use the term) extinct, it has
choked itself up, it is mastered by soft mud. The interior
ice moves slowly towards the coast, more like pitch on a
roof exposed to the sun, than like a solid body forced for-
wards, and the glacier finds its way into the deep fiord
simply because it affords an easy outlet. . . . Almost all the
lower transported material is pushed into the fiord, the mud
floats away, most of the boulders and sand remain, and the
first iceberg launched in the fiord commences the slow but
certain checking of the glacier, for it has not power to move
its own loose material. The glacier blocked up, the edge of
the ice retires inland by melting, and a stream of water brings
down the sand and mud left on the land, making the fiord still
shallower. The inland ice now seeks another outlet, and then
a deep fiord, perhaps previously clear of ice, becomes encum-
bered with icebergs. The ruins of Scandinavian villages may
be seen in fiords now almost inaccessible for icebergs, and at

the heads of fiords now unnavigable in boats, from deposits from the glaciers.

The sides of many glaciers are of soft material, sandstone with coal, blacklead, etc. Why were these not ground away? Then the shape of some fiords is incompatible with the theory of ice cutting, for ice could not cut in opposite directions. Mr. Taylor then refers to a fiord the second south of Arksut, in which there is a very sharp elbow, so that it has two arms, one of which is closed by a barrier, and this barrier exists at the point where its force must have been greatest. It consists of crystalline limestone 20 feet broad, and does not show the slightest marks of ice. . . . "I maintain," he concludes, "that the fiords were in existence prior to their invasion by the glaciers. As to their origin, I think the geologist in Greenland will see in the immense number of erupted dykes and upheaval, distortion, and fracture of the older stratified rocks, a cause more adequate to the effect than ice hollowing."[4]

Referring to the researches of Jensen, etc., in South Greenland in 1878, M. Johnstrup says, "Quoi qu'on n'eût pas constaté en Grönland un seul fait qui semble conforma la théorie qui les fiords et les vallées devraient seulement leur origin à une erosion des glaciers, il est cependant incontestible que ces derniers ont élargé et poli les vallées qui existaient avant leur apparition, et à travers lesquelles ils ont passé. Meddel, om Grönland, etc., I. 194.

Pettersen, whose minute knowledge of the Northern Norwegian fiords was unrivalled, says, "We therefore come to the conclusion that the basin of the Bals fiord, viz., the Tag lake depression and the Sag valley, cannot be the result of the erosive action of the inland ice, but that it existed prior to the glacial age, and that, in fact, the depression in question was the cause of the ice stream taking this course."[5] Again he says, "I have, therefore, after the most careful researches here, yard by yard, and extending over many years, come to the conclusion that the Bals fiord is not of glacial origin, but formed an incision or depression in the mountains of older origin than the glacial age, and this conclusion I believe may, in the main, apply to the question of the formation of all fiords in the north of Norway; but whether it is applicable

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[4] Proceedings R. Geog. Soc. xiv. 1870, 156-158.   [5] Nature, xxxii. 179.

to all fiords in the whole of Norway, I shall not attempt to
answer. There may, however, be reason to assume that the
explanation of the fiord formation, in parts which have lain
under an earlier glacial age as being of glacial origin, is rather
based on speculation than such careful and minute researches
as those I have referred to here, and which may perhaps con-
tribute to prove the true theory." [6]

It is sometimes said that fiords only exist in arctic lands.
This is not so. In the arctic lands they have been cleared of
débris by the glaciers, while elsewhere they are choked. Thus
fiords exist in Asia Minor, in Dalmatia, in the Asturias in
Spain, in Brittany, in the desiccated Gulf of Carentan in
Normandy, and in Granada between the Atlantic and the
lake of Venezuela. In Provence, in the south-east of France,
says Falsan, there are certain fiord-like fissures known as
calanques. He adds that if the land were depressed so
that the basins of the Rhone and Saone were occupied by the
sea, the greater part of the secondary valleys would be con-
verted into fiords such as the valleys of la Durance and Verdon.
The fissures or *clus*, by which the western flanks of the
Maritime Alps are marked, are singularly like fiords. Similar
openings occur in the valleys of the Tarn, the Tarnon, and
the Jonte. These fiords occurring where ice action on a large
scale is out of the question, show that the limitation of the
phenomena of fiords to the higher latitudes is not justified.
Another remarkable case is the valley of the Isère, especially
between Voreppe and Grenoble, and in all the Grésivaudan
and in other parts of the valley, of which M. Falsan says, " si
ces dépressions étaient donc envahies par la mer, ce serait une
grandiose ramification de canaux, de lacs, d'iles, qui rappelle-
raient les sites de la Norvège. En effet sur le prolongement
septentrional du Grésivaudan, un vallée étroite, profondé-
ment enfermée entre de hautes montagnes, permet à la
vallée de l'Isère de s'anastomoser avec celle du Rhône, comme
les fjords de la Norvège le font si souvent entre eux ; et une
partie de cette étroite vallée est même occupée par un lac
profond, le lac du Bourget, ce qui contribuerait à rendre la
ressemblance plus frappante." [7]

If the sea were to invade the basin of the Rhone from

[6] Id. 180.   [7] Falsan, 114-117.

Lagnieu as far as the recesses of the Valais, following the defiles of Vertrieux, Pierre Châtel, Bellegarde, Fort-L'Ecluse and following the lake of Geneva as far as Bex, Martigny and Sion, as well as the valleys of the Fier, the Arve, the Dranse, etc., we should have a country of lakes and fiords like those of the north. Reclus similarly compares with the Norwegian fiords the various narrow valleys in which the lakes of North Italy lie, and the deep gulfs on the coast of Brittany, such as those of Brest and Morlaix, and the now dry gulf of Carentan in Normandy. Similar fiords occur in the south-west of France, as the picturesque gorges of Ollioulles, near Toulon, and the small bays near Cassis, east of Marseilles. M. Falsan says of these latter : " Ce pays n'ayant j'amais été soumis à la glaciation, il est donc impossible de trouver les traces d'un glacier au milieu de ces aiguilles, de ces pyramides, de ces colonnes, de ces voûtes en surplomb, et cependant le fiord existe il n'y manque que le vernis c'est a dire le polissage que donne toujours la progression d'un glacier. " [8]

On the coast of Liguria the same phenomenon occurs, but is disguised by the fiords being filled up with soft deposits. If these were removed, the coast from Nice to Cape St. Martin would resemble the coast of Norway. The same thing may be said of the coast of Dalmatia and of Asia Minor.

Again, Viollet le Duc has shown that not only cannot ice cut small fissures in rocks, but that above the gorges of Trient, the beds of the Salvan, although very hard, are much *moutonnée*, showing a big glacier once occupied them, but this ice could not penetrate the narrow gorges of Trient, Birard, the Via Mala, the Tête Noire, etc.[9] Nor can ice cut out fissures with perpendicular or overhanging sides ; only with synclinal ones. That the ice has polished and scored and worn the general contours of the fiords in the north is plain, but this has been merely to make itself an easy bed and to reduce friction to the smallest point, and I quite agree with Falsan when he says, " La glace aurait-elle pu agir comme une lame de scie sur une pierre tendre ? Nous ne pouvons le croire. Nous supposerions plutôt que la puissance érosive de la glace pourrait plus facilement ouvrir de larges vallées dans

[8] Op. cit. 119, 120.   [9] Falsan, Op. cit. 143.

un massif montagneux que creuser ces cluses, ces canons étroits, profonds et sinueux qu'on appelle des fiords."[1]

Let us now turn to another part of our subject which has been much discussed, namely, the glacial erosion of lakes. The theory of the glacial origin of lakes has been presented in two forms. First, by Ramsay, who assigns to ice the actual excavation of the lake basins, and, secondly, by Mortillet, who merely claims for it the power of cleaning out the detritus and gravel, etc., which had accumulated in pre-existing hollows and valleys.

In his paper on the " Glacial Origin of certain lakes in Switzerland," etc., in 1862, Ramsay argues, that the Alpine lakes do not lie in areas of special subsidence, nor in gaping fractures, nor in simple synclinal basins, nor in hollows of watery erosion. He then goes on to argue that the areas now occupied by lakes, both north and south of the Alps, have all been covered with glaciers, and there is, therefore, a connection between them that cannot be accidental; secondly, that no tertiary beds between the miocene and the glacial beds exist in Switzerland, and had the hollows existed before the ice age, we ought, but for some denuding agent, to still find traces of fresh-water deposits, perhaps of the age of " the crags " of which none exist.

Having argued that the lake of Geneva does not lie in a synclinal trough, nor in an area of subsidence, nor in a line of fracture, nor in an area of aqueous erosion, he continues, " We have only one other great moulding agency left by which to modify the form of the ground, namely, that of ice. . . ." " It required a solid body grinding steadily and powerfully in direct and heavy contact with and across the rocks, to scoop out deep hollows, the situations of which might be determined by unequal hardness of the rocks, by extra weight of ice in special places, or by accidental circumstances, the clue to which is lost from our inability perfectly to reconstruct the original forms of the glaciers. . . . Most of the lakes are broad or deep, according to the size of the glaciers that flowed through the valleys in which they lie." Turning to America, he concludes " that its lake basins could only have been scooped out by true continental ice like that of Green-

[1] Id.

land, for the lakes are universal in all the ice-worn region."
" For Wales and many parts of Scotland I can answer that by
far the greater proportion of the lakes lie in rock basins of
truly glacial origin." "The islands of Loch Lomond are
mere *roches moutonnées*." He speaks in a similar way of the
lakes and fiords of Norway, and the salt-water lochs of Scotland,
as true rock basins scooped out by the passage of glacier ice." [2]
These views Ramsay elaborated in his " Physical Geography,"
and in the 28th volume of the *Phil. Magazine.*

Writing again in 1869 on the " Old Glaciers of Switzerland
and North Wales," he urged that the excavation of deep
hollows in solid rocks was due to a weight of superincumbent
ice, pressing and grinding downwards and outwards over
high, flat and sometimes broad watersheds and tablelands,
during that period of intense cold which produced the old
glaciers.

Ramsay's argument, as Mr. Ball says, is virtually this:
Each of the great Alpine lakes lies in an area once covered by
a glacier; no satisfactory explanation of the existence of
Alpine lakes has yet been given; the glacier, considered as a
mechanical agent, is competent to scoop out the rock basins
in which the Alpine lakes generally lie, therefore the lakes
have been formed by glaciers. Mr. Ball says, very truly, that
we are not entitled to argue from our own ignorance to the
admission of a new agent, until its competency shall be proved
by direct evidence.[3]

As Mr. Whymper and others have remarked, Ramsay does
not trouble himself to prove how glaciers could excavate
hollows, he merely urges that these mountain lakes occur most
frequently where glaciers can be shown to have existed, and
that it is possible they may have excavated the softer portions
of their beds, leaving the rest more intact. In regard to the
former argument, Mr. Whymper says it would be as reason-
able to attribute the existence of the Arctic Ocean to the
work of the icebergs that float in it; while in regard to the
latter one, it is actually contrary to the facts. We have
no evidence whatever that ice in its movements discriminates
between hard and soft bodies, but just the reverse. The par-
ticular feature in which ice differs from all other erosive agents

is in its planing down stones of all degrees of hardness to one level.

As Lyell says, " Glaciers do not and cannot dig away soft places. The hollows in which pools occur would be obliterated, and not deepened, if glaciers worked sufficiently long." He remarked further " that when a glacier has retired in historical times, no basin-shaped hollows are conspicuous, cup-and-saucer-shaped cavities are wanting." [4]

Whymper has shown this very clearly, and he quotes a notable case he had seen at Claushavn in Greenland, where the gneiss was levelled and polished for hundreds of yards. In these gneissic rocks were two great quartz veins, each three or four feet thick, which ran roughly parallel to each other about eighty yards apart. The glacier had passed over them at an angle of 10°. " Upon this quartz," he says, " my hammer danced and rang and made scarcely any impression. I chipped away the gneiss without difficulty. Yet the quartz, instead of standing up in ridges, was cut down to the same level as the gneiss ; the keenest scrutiny could not detect the least difference." [5]

When Ramsay read his paper in 1862, it was immediately answered by Lyell, who says, *inter alia*, " that several of the great Swiss lakes are by no means in the line of direction which they ought to have taken, had they been scooped out by the pressure and onward movement of the extinct glaciers. The lake of Geneva, for instance, would have been prolonged from the termination of the upper valley of the Rhone, towards the Jura, in quite another direction."

He next urges that inasmuch as the upper layers of the ice flow faster than the lower, and that, as according to Ramsay, in the Lago Maggiore, which is more than 2000 feet deep (797 metres), the ice had to descend a slope of about 3° for the first twenty-five miles, and then to ascend for the last twelve miles (from the deepest part towards the outlet) at an angle of 5°, " it is for those who are conversant with the dynamics of glacier motion to divine whether, in such a case, the discharge of ice would not be entirely effected by the superior and faster moving strata, and whether the lowest

[4] " Elements," 6th ed., 170.
[5] Whymper " Scrambles in the Alps," 338-9.

would not be motionless, or nearly so, and would therefore exert very little, if any, friction on the bottom."

He then comments on the entire absence of lakes of the first magnitude in several areas, where they ought to exist if the enormous glaciers, which once occupied those spaces, had possessed the deep excavating power ascribed to them. Thus the ancient moraine of the Dora Baltea, the product of a glacier derived from Mont Blanc and Monte Rosa, one hundred miles apart, emerged from the narrow groove above Ivrea, and deployed on the plains of the Po. It advanced over marine pliocene strata no harder than the miocene sandstone in which the lakes of Geneva and Zurich, etc., are excavated, why, then, did it not excavate basins like the lakes of Maggiore or Como, instead of merely giving rise to a few ponds above Ivrea? . . . There ought to have been another greater lake in the space now occupied by the moraine of the Dora Riparia, between Susa and Turin. Gastaldi has shown that all the ponds in that area consist exclusively of what M. de Mortillet calls morainic lakes, i.e. lakes caused by barriers of glaciers, mud and stones.

In answer to Ramsay's argument that no fresh-water strata exist in the Alps between the miocene beds and the glacial ones, and that therefore the great lakes could not have existed in pre-glacial times, Lyell points out that the fresh-water shells, which occur in the beds at Utznach, which were a delta formation, show that the lake of Zurich, at all events, existed before the glacial period, since the beds are overlaid by erratic blocks. Similar fresh-water formations occur at Wetzikon, near the Pfaffekon lake, at Kaltbrunnen, at Bruckberg, and Morschweil between St. Gall and Rorschach, which, he says, are all pre-glacial, like the forest bed of Cromer.

Lyell concludes that such lakes are really due to unequal movements of upheaval and subsidence.[6]

In the discussion on a paper by Mr. Godwin Austen, before the Geographical Society, in 1864, Dr. Falconer contrasted the Alps and Himalayahs. The former are girdled by a series of lakes jutting out from them, wherever a great valley projects itself from the chain at right angles to its strike. These lakes are invariably narrow, and some forty or

[6] " Antiquity of Man," 309-319.

fifty miles long, as notably in the case of Maggiore, Como and Garda, and they invariably radiate out at right angles to the strike of the great chain of the Alps, which make a curve from the Pennine to the Rhætian Alps. Each of these lakes is fed by a considerable river proceeding from a high ridge of the chain.

In the Himalayahs, on the other hand, and other tropical mountain chains, these lake phenomena are invariably wanting. Great rivers like the Indus, the Chinab, the Sutlej, etc., debouch into the plains into valleys of much greater importance than the Alpine ones, but they are not connected with lakes.

After many years of study of the cause of this, both in India and Europe, and repeated visits both to the Himalayahs and the Alps, he could not accept the views of Ramsay. Taking the case of lake Maggiore, the glaciers of the Ticino must have descended a steep incline into the lake, which is about fifty miles long, eight or nine wide, while its bed sinks to 2600 feet below the sea level. The river which drains it flows out at about 600 feet above the sea level. The glacier, by Ramsay's hypothesis, must have actually dived into the bowels of the earth, 2000 feet below the coast of the Mediterranean, and then have risen up again along an incline at the rate of about 180 feet per mile. The mechanical difficulties in such a view are insurmountable.

Falconer argued that the difference between the two chains is, that the fissures now joining lake basins were, in the glacial period, in the case of the Alps, filled up with ice, and consequently the *débris* passed over them and did not fill them, while in the warmer conditions prevailing in the Himalayahs the ice did not go down so far, and the fissures were consequently filled with *débris*. He thus accounts for there being lakes north of the Himalayahs and not south of them.[7]

Murchison, writing in 1864, says he agrees with Falconer, with Studer and Von der Linth, " that the chief cavities, vertical precipices, and subtending deep, narrow gorges of the Alps, have been originally determined by movements and openings of the crust, whether arranged in anticlinal or synclinal lines,

[7] Proceedings Royal Geog. Soc. vol. viii. 38–41.

or not less frequently modified by great transversal or lateral breaks, at right angles to the longitudinal or main folds of elevation and depression;" or, as he puts it later, "the deep cavities in which the rivers and lakes occur are due either to dislocations producing abrupt fissures, or to great foldings of strata leaving openings upwards where the tension has been the greatest, openings which were enlarged by powerful denudations." Escher Von der Linth, in a letter to Murchison, points out that the Rosenlaui glacier, which descends from a great height, does not enter a low deep narrow gorge of the valley, but forms a bridge over it, and so it is to be inferred that as the ancient glacier did not excavate the gorge, still less did it excavate the valley in which the present glacier is embosomed. Murchison himself urges that in valleys of slight descent the glacier is usually separated from the subjacent rock or vegetable soil by water arising from its melting. In the steeper valleys we don't find that either the sides or bottoms of the upper gorges afford proof of wide erosion, but only the peculiar fashioning of the flanking surfaces, or that rounding and polishing called *moutonnée*, accompanied with striations; on the contrary, in gorges wherein the largest glaciers have advanced for ages, we meet with islands of solid rock and little bosses still standing out, even in the midst of valleys down which the icy stream has swept. Speaking of the case where a lake lies in a direction at right angles to the discharge of an old main glacier, as that of Geneva, which travels from east to west, while the detritus and blocks passed from north to north-north-west, he asks, Where was the mountain by which the glacier could be so deflected to the west, that it could channel or scoop out, on flat ground, the great hollow now occupied by the lake? and after effecting this wonderful operation, how was it propelled upwards from this cavity on the ascent to great heights on the slopes of the Jura? Of the lake of Constance which he visited in company with M. Escher Von der Linth, who knew every inch of the ground, he says, he had never seen on the north flanks of the Hohe Sentis, which presents its abrupt, precipitous, and highly dislocated and contorted jurassic and cretaceous rocks to the lake, with terraces of miocene deposits at great heights, signs of the action of a great glacier which could

in its descent have so plunged into the flat region on the east and north, as to have scooped out the cavity in which the lake lies. Then there are no traces again of those great old moraines, the level country to the north of the lake being free from them.

In Spain again, as he had been informed by M. de Verneuil, the large depressions on either side of the granite mountains present exactly the appearance which a theorist might attribute to excavation by ice, and yet, however these cavities were formed, it is certain that no glacier has ever existed there. Nor again has ice ever acted on the steep mountains of Murcia, where deep excavations and denudations are seen upon the grandest Alpine scale.

In the Urals, where no traces of old glaciers exist, we meet with both longitudinal and transverse deep fissures, in some of which lakes and in others rivers occur. On their eastern flanks we find a succession of depressions filled with water, without a trace on the sides of the bare and hard rocks, which subtend these lakes, of any former action of glaciers.

According to Collomb, none of the glaciers of the Rhone, Aar, Chamounix, the Allée Blanche and Zermatt produce any excavation in the lower grounds over which they pass. That of Görner, which is advancing, affects very slightly the surface of the meadows on which it proceeds, and does not penetrate into the soil. The glacier of the Lower Aar, in pushing over old moraines and gravels, scarcely damages their material, but slides over them, leaving them covered with mud and sand, but not excavating them. The Rhone glacier advances on a gravelly substratum, in which it does not form a channel. On the surface of the ground from which the glacier named Bossons has recently retired, there is not the smallest sign of excavation. Old moraines repose directly on loose quaternary beds containing remains of the Mammoth, how could they thus move if they had such excavating power as to scoop out deep valleys in hard rocks ? If the glaciers excavated lakes Garda and Maggiore, why did they not do similar work at Ivrea in advancing through the Val d'Aosta, or at Rivoli, on their way from Mont Cenis towards Turin ?[8]

Again he points out that the narrowness of the channels

[8] Phil. Mag. xxvii. 481.

through which glaciers have been thrust for countless ages, is in itself a demonstration that the ice can have done very little in widening the gorge through which it has been forced, and where of necessity it exerted by far its greatest power. In other words, the flanking rocks of each gorge have proved infinitely more stubborn than the ice, and its imbedded stones which have merely served as gravers and polishers of the granites, quartz, rocks, porphyries, slates, marbles, or other hard rocks, among which the frozen river has descended, and if such has been the amount of influence of advancing glaciers in the higher regions, where the body descends with the greatest power, how are we to believe that when this creeping mass of ice arrived in low countries, it could have exerted a power infinitely greater than that which it possessed in the higher regions ? [9]

C. Martins says, " the mode of action of the ancient glaciers must have been that of modern ones, except that they acted on a larger scale." He accompanied a large party of naturalists to a great glacier in 1864, and several members, including Mr. Desor, remarked that the glacier did not plough the soft materials over which it passed, and it was also noticed to pass over the boulders lying in the bed of its torrent. All were struck also by the horizontality of the beds over which it had travelled. " Ainsi donc," he says, " un glacier ne pénétre pas dans un terrain meuble à la maniere d'un soc de charrue que entame la sol et l'affouille ; il s'agit comme un grand polissoir qui le ruvelle." [1]

Ramsay replied to his critics in a paper in the *Phil. Magazine* for 1864. In this he challenged them to produce the valley-faults to which they attribute the lakes, and says that only when all the faults and curvatures possible are actually laid down, and when geological sections on a true scale have been run across the Alps, will it be possible to reason with precision on the denudation of the mountains. This is assuredly shifting the burden of proof. In regard to the elbow in the lake of Geneva, he affirms that the glacier he appealed to was not only the Rhone glacier, but a great tributary glacier that came from Chamounix. He further says, that

[9] Journal R. G. S. xxxiv. clxiii.-clxxv.
[1] " Revue des Deux Mondes," l. 88 and 89.

during the glacial period, all the higher regions south of the lake must have maintained glaciers, and filled the valleys that run north, and that the great glacier meeting with the Jura was deflected in the direction of least resistance, i.e., at right angles to the general pressure. Hence, the general form and trend of the lake of Neuchatel. As to the excavating power of ice, he points to the glacier mud, to the striation and deep grooving, the mammillation and glassy polish of the rock surfaces. In regard to the rocks existing in the midst of old glacier valleys, he does not meet the argument, but merely says they cannot be explained by the dislocation theory, though they might by water erosion. As proof that glaciers can move up hill, he cites the roches moutonnées : " If a glacier can round, polish and cover with striations the rocks over which it passes, if flowing from its caverns, it can charge rivers thickly with the finest mud, then it can wear away its rocky floor and sides."

He admits that certain glaciers override their moraines, but denies they would do so if they were on the scale of those of the glacial age; "the weight of a thick glacier must exercise a prodigious abrading power." Hence, he argues, the old glaciers could excavate lakes when rocks of unequal hardness came in the way, or when from special causes the pressure of the ice was unusually great on certain areas, or when confluent glaciers met at certain points. Where the slope was steep the glacier would flow like a torrent, and would not cut out for itself a basin-like hollow ; but when a monstrous glacier entered a flat plain, there the ice had time to select soft places for excavation, and there, if from the confluence of large glaciers, or from other reasons, the downward pressure of the ice was of extra amount, the excavating effect must have been unusually great on special areas, and led to the formation of rock-bound hollows. He does not meet the argument that lake basins do not occur where they ought, according to his theory, but says : " I have attempted to explain why the rock basins are present, and not why they are absent" ; and he can only suggest that it may be that some of the alluvial flats of the valley are lake hollows filled up. The islands in Lake Maggiore, Loch Lomond, etc., he describes as mere roches moutonnées, and he urges that the length and inclination of a slope up which a glacier can ascend, depends simply on the

thickness of the ice, and on the amount of the propelling power behind, due to the weight and mass of the descending glacier and the average angles of the valley behind the point whence the upward ascent begins.[2]

In a later paper devoted to meeting Lyell's views, Ramsay adds little to what he had previously urged. He does, however, argue that the erosive effect of a glacier would be in a measure proportionate to its thickness, and that it would have a tendency to pile itself up just outside the mouth of the valley, to there attain an unusual thickness, and thus exercise an extra erosive power. As it moved away from the valley it would thin by melting; hence the weight being lessened, the waste of underlying matter by erosion would also decrease towards what are now the mouths of the lake basins, thus accounting for the gradual slope of the bottoms of these lakes towards their outflow, and accounting for the contours of their beds.[3] He also contests Lyell's opinions that there were lakes at all geological periods, and asks for proof of this statement.[4] The rest of the paper is chiefly devoted to arguments against lakes which are situated in natural depressions being caused by dislocations of the rocks, etc.[5]

In regard to one of these arguments, Mr. Whymper most conclusively urges that, on debouching into the plains, glaciers spread out and do not heap themselves up.

In his address to the Geological Society, in 1865, Mr. W. J. Hamilton said, *inter alia*, that it seemed improbable that a glacier, after emerging from the narrow valley in which it has been confined, and having entered a vast plain, where little or no lateral pressure can be exerted, and when it has reached a state of comparative if not absolute rest, should, by the mere force of its enormous thickness, exercise such a pressure upon the rocks below as to excavate a basin, in the solid rock, of several hundred feet in depth. I am not aware that Professor Ramsay himself has anywhere stated that, supposing the glacier were at rest the mere vertical pressure could produce this effect upon the rocks beneath. This statement has, however, been made by Dr. Haast in his account of the glaciers of

[2] Phil. Mag. 4th series, xxviii. 293-307.
[3] Phil. Mag. 4th series, xxix. 287.
[4] Id. 288.   [5] Id. 288-298.

New Zealand. Turning to the Lake of Geneva, Hamilton shows that the ice which is supposed to have excavated it must have been in some places 3764 feet thick, and extended over a hundred miles in length from Geneva to Solothurm, and thirty miles wide between the Alps and the Jura, and he continues : " The greater portion of this glacier must have come down the valley of the Rhone by Sion, Martigny and Bex, and this valley, particularly near its termination, cannot be more than two or three miles in width, in many places not so much. The inclination from Sion to Villeneuve is very slight ; and although it is probable that the Alpine chain may have had a greater altitude during the glacial period than at present, there is no reason for supposing that the angle of inclination of the main valley down which the great glacier advanced was greater than it is now, although that of the smaller valleys, which also contributed their quota to swell the moving mass, may have had a somewhat steeper inclination. "Admitting all this," he says, "and remembering the thickness of the ice covering the plain, I am at a loss to look for that power-ful agency, that *vis a tergo* by which this comparatively small mass could push forward, at the very lowest imaginable rate, the vast field of ice covering 3000 square miles of the great plain of Switzerland, and yet with such irresistible energy and in a direction at right angles to its principal line of move-ment, as to cause it to scoop out the rocky basin in which the Lake of Geneva now lies." To this argument Ramsay replied that the ice in the Rhone valley was prodigiously swelled by the great tributary glacier of Chamounix, which, descending from Mont Blanc, filled a valley some fifty miles in length, and joined the Rhone glacier near the lower end of the Lake of Geneva ; and further, that during the cold of the glacier epoch all the higher region south of the lake must have maintained its glaciers and filled the valleys that run north. Mr. Hamilton retorts (assuredly with great force) that the effect of this Chamounix glacier, instead of reinforcing the Rhone glacier, must have checked its momentum and progress, if it had any, towards the south-west, and tended to drive the two together in a line of mean result against the flanks of the Jura ; but putting this aside, he asks, " Is it within the range of physical possibility to suppose that a mass of ice, two or three miles in width,

should have had the power of forcing onwards, over an uneven bottom, a mass occupying a space of upwards of 3000 square miles, and moreover of scooping out a basin in the solid rock to a depth of 900 feet? . . . If a certain amount of pressure from above did cause the glaciers to descend the valley from the high Alpine regions, this descending mass, or rather the upper portion of it, on reaching the icy plateau of the plain which must have resisted its pressure, would rather have flowed over the surface and have spread itself over the comparatively quiescent portion below. . . . If the glacier did not move, but excavated the depression when still, how was the abraded material removed?   Lastly, the tremendous weight of the postulated ice would, when in motion, cause so much friction and develop enough heat to reduce the ice in contact with the hard rocks into a liquid state, and thus cause a stratum of water to be introduced between the rocky bed and the ice, which would act as a kind of cushion, and thus prevent it from exercising its full abrading force.[6]

Mr. Ball asks why, if Ramsay's view is reasonable, there are no large lakes in the valley, once filled by the glacier between Susa and Turin, or the still larger one that once filled the Val d'Aosta, or in the Dauphiné Alps, the Tyrolese, Salzburg, Carinthian and Styrian Alps. Again, such lakes as that of Lugano or Zug, if excavated by the ice, must have been by very slender streams of ice ; and, he continues, "the boldest speculator in glacial theories must hesitate to assert that any agency was then present that could even tend to excavate a trench 919 feet below the present level of the lake, in that of Lugano, and 1279 in that of Zug." [7]

If we suppose a glacier of great thickness to lie on a level surface many miles in extent, it is easy to see that the movement of the lower surface of the ice would be completely stopped by the flowing of the upper over the lower portions of the mass. It is forgotten that the resistance offered by friction would be increased to an almost infinite extent, while the resistance of the substance of the glacier to internal re-arrangement is confined within moderate limits.   The greater the pressure invoked, the greater would be the resistance to onward movement, since the ice would be forced into every

	[6] Op. cit. xxi., lxxxvi.-xc.		[7] Phil. Mag. 4th series, xxv. 88-90.

inequality of the bed; *a fortiori* must this have been true when the glacier is supposed to move up a slope.

Mr. Ball asks how a glacier like that of the Rhone, after it had flowed out of its native valley, was able to excavate a rock-basin fifty miles and more in length, twelve miles broad and 1000 feet deep, while at the point where its mechanical power was at the maximum, in the defile of St. Maurice, it has failed to cut through rocks of no extreme hardness a more spacious opening than that between Bex and Martigny. It is quite an under estimate to assert that the glacier must have moved through that defile at a pace twenty times greater than its rate of motion anywhere over the level of the lake; yet we see how limited its effect has been upon the containing walls of the valley.

How, again, on this theory, are we to account for the presence in most Alpine lakes of projecting points of rock, and sometimes of rocky bays and caves in the direction to which the glacier flowed; such as the rocky promontory where stands the Villa Corconati on the Lake of Como. The glacier, whatever its efficacy as a tool may have been, worked in one direction only, and cannot have scooped out hollows in a direction contrary to its own current.

These arguments seem to me conclusive, and I cannot understand on what physical principle it is argued that excavation by ice is possible except as in the similiar instance of water, when it falls from a height and by its dead weight excavates basins at the feet of water-falls, etc., or in some cases washes out soft débris and thus forms pools. A glacier by itself, and acting independently of any rock masses attached to its sole, would simply smooth down asperities in its way which retarded its movement, and, having once secured a smooth and easy channel, the effect of friction would be virtually *nil;* and I cannot understand under what conditions it would dig out such large lakes as those for instance of North Italy, when it had moved a long distance from the Alps to which it owed its motive power. There seems no force available anywhere for the purpose. Writer after writer has remarked that the lakes and basins in question occur only near mountain centres, but why, on their own showing, this should be so, I fail to see. If the ice could move

for hundreds of miles over level plains, thus proving that it retained its *vis a tergo*, why should the lakes in question be limited to mountain regions? I agree with Mr. Irving, that the only cases where glaciers do actually excavate or have excavated basins, are probably to be limited to those where " successive portions of a glacier descend a vertical or very nearly vertical precipice, and where impact may do the work of excavation to such an extent as is represented in the excavation of many small rock basins, such as some of those which lie upon the Bernina Pass, or at the foot of the precipices of Snowdon."

The Duke of Argyll says : " I am at a loss to understand how glaciers could stop their onward motion and expend their energies in digging deep holes far below the general slope upon which they lie and upon which they move. Take the case of Loch Lomond. Here we have the great valley, the bottom of which is at some places 600 feet below the level of the sea. If the floor of that valley was at a much higher level when the glacial period began, ice moving down it from the higher ranges of Glenfalloch would follow the line of least resistance to the open country and the Firth of Clyde. There is no conceivable circumstance, except one, which could cause the glacier to expend its force in the direction of excavation instead of the direction of advance. The one circumstance which might possibly explain the descent of a glacier into such a depression, is that the depression existed before, in which case of course it would be occupied by ice just as it is now occupied by water. But what would be the behaviour of that portion of the glacier which had been so embedded in a rocky hole? Would it move as freely as a glacier lying on a level or on a descending slope? Is it quite certain that it would move at all? Would not the friction and resistance be so enormously increased as to bring this lower portion of the glacier to rest, and allow the upper part of it to slip over the lower part, as we know it actually does even when the lower ice is not thus imbedded? I propound this as a question of dynamics. It is quite certain, I apprehend, that a rocky depression of some certain depth and thickness would hold absolutely fast any mass of ice lying in it, and that the ice in motion over it would have to part company altogether with

this embedded portion, even as the upper part of the Mer de Glace does now part company from its own base by a double rate of motion.   And has any calculation been made of the gradient, so to speak, at which ice would thus be brought to a standstill, and would thus absolutely cease to work even as an abrading agent?   Until such calculation has been made, the theory which ascribes the digging out of the deep rock basins to the action of moving ice is a theory which omits altogether to take into account an essential element in the problem which it aspires to solve."[8]

"It is remarkable," says the Duke again, "that the rock basins attributed to glaciers do not, for the most part, lie in the parts of valleys which are narrow or obstructed, but, on the contrary, in general where valleys open out to the broadest extent, and either debouch upon the plains, or enter a country with minor hills.   This is the case with the Lakes of Geneva, Maggiore, Garda, Como and Lugano, and in our own country with Loch Lomond and Loch Awe.   I may add with regard to the thousand little lakes or tarns which are common in the highlands, and literally honeycomb the whole face of the country in some parts of Sutherland, that very many of them occupy positions which it would be very difficult to explain by any reasonable theory of the spots which would be selected by glaciers for the exercise of their digging powers. Many of them in Argyleshire are on the very tops of hills, and almost all of them in hollows close to the summits, situations where, on any possible theory, the weight and working power of land-ice would be at its minimum.   On the whole, therefore, I am inclined to think that, however much enormous glaciers may have deepened the hollows now occupied by lakes, they cannot have dug them out in any case in which they exhibit any considerable depth below the general level of the slope in which the glacier lay. . . . Nor can I see any evidence of late basins having been excavated by ice in the fact that the striations observable along the shores are generally parallel with the longer axis of the occupying links. This would of necessity be the case on any theory, because, whether those striations were made by glaciers, or only floating ice, and whether the valleys were first cut out by ice or

[8] Q. J. G. S. xxix. lxx. and lxxi.

only occupied by it after they had already formed, the direction of movement would be equally parallel to the valleys as they now exist." [1]

The Duke further points out that in the case of Loch Lechan in Argyllshire, which is a mile long, and at one point eighteen fathoms deep, there is no hill near, which could form the gathering ground of a glacier capable of doing excavating work. Again, it is separated from Loch na Craig by a very narrow ridge. The latter lake is nine fathoms deep, of a circular form, and about 150 feet distant from Loch Lechan. How did the ice which cut out this lake in granite, leave the narrow dividing ridge standing? There is nothing to show that this deep little hole lay in the route of any conceivable glacier or ice sheet. Again, Loch Naume and Loch Shalaga are both on the very tops of granitic hills, one of them occupying the summit level between Loch Awe and Loch Fyne. These are likewise not in the path of any glacier, and in positions in which it is difficult to conceive that ice of any kind could have employed itself in digging holes. [2]

Mr. Bonney urges that the great majority of Alpine valleys show no tendency to lake basins, in places near, but above, the present lakes, where the conditions must have been as nearly as possible the same as over the area of the basin; that is to say, if you ascend a valley from the head of a lake, you often find it throughout a true valley of river erosion, only very slightly modified by glacial action. Surely the glacier would have tried " its prentice hand " now and then before for example, excavating Como. [3]

In a paper on Alpine Lakes, in which he examines especially the lakes of the Eastern Alps, the same author lays down two postulates :

I. Supposing a glacier able to excavate a lake basin, there must exist above the head of each lake a district capable of producing a glacier.

II. Under no circumstances can glaciers, especially near their heads, erode considerable precipices or slopes approximately vertical.

The former proposition, he urges, is as self-evident as

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[1] Q. J. G. S. xxix., lxxiii. and lxxiv.   [2] Q. J. G. S. xxix. 509-510.
[3] Geol. Mag. 1876, 376-7.

saying that a large river cannot exist without a large catchment basin; and as to the latter, he says that a glacier planes, though with varying force, but cannot dig.

After a detailed examination of the lakes, he concludes: "We have here a district whose lakes though generally inferior in size to those of Switzerland, are more numerous in proportion to its area. Some of them, to say the least, lie in the neighbourhood of glaciers; others have doubtless been occupied by them. Two of them (the König See and the Hallstädter See) differ from all the Swiss lakes in lying practically at the heads of valleys, just where we might expect a glacier-worn basin to exist, if it did at any place; and yet the forms of these two are those which it is most difficult to reconcile with any theory of glacier erosion. Again, among others, the Fuschel, Wolfganger, Mond, and Atter Seen, lie where it is almost impossible that any very large ice streams can have passed, or great pressure been exercised; yet one of these and that the one most completely protected from glacial action, is the largest and deepest in the whole district, deeper than most of the Swiss lakes. I think, then, that when applied to this region, the theory of glacier erosion fails to account for the phenomena. This being the case, we are led to doubt whether the explanation, which seems more specious in the West and Central Alpine regions, can, when we regard the whole chain of the Alps, be regarded as applicable even there." Reverting to his observations on cirques, he says, "Further examination of one of those mentioned, and of several others has fully confirmed me in this opinion, and convinced me that the excavating agent to which they were assigned (the erosive action of streams) is the true one—convinced me also that all the principal features of the Alps are of earlier date than the glacial epoch." In the discussion on this paper, Professor Ramsay admitted that the cliffs referred to by Professor Bonney round some of these lakes were of pre-glacial origin and due to meteoric causes. Mr. Koch said that where the planing action of glaciers in ancient times was visible, it appeared to him to have acted in straight and not in curved lines. The Duke of Argyll said he thought the author's contention, that most precipices, as those surrounding the König See, could not have been formed by the action of glaciers,

was substantiated; and he cited Mr. Kinahan as having stated that the greatest depths of Loch Lomond coincided in position with faults which could be observed on the surrounding country."[4]

In a paper on the Upper Engadine and the Italian valleys of Monte Rosa, read before the Geological Society, in May, 1874, Mr. Bonney again urges that the difference in contour between valleys excavated by water and by ice, is that between V and U, and if a lake basin has been cut by a glacier, the valley above must be more or less U-shaped.

In regard to the Lake of St. Moritz, after elaborately examining its surroundings, he concludes that the lake is a true rock basin, but the physical features of the district in no way lend themselves to a theory of glacier erosion. They rather seem to be pre-glacial in origin, and to have been only superficially modified by ice action. In regard to the Selser See, he similarly concludes that, with all the other physical features of the head of the Inn valley, it is anterior to the glacial epoch, and has not been much altered since. . . . The rocks are rounded by glaciers, but there is nothing to suggest any scooping action, no great modification of the outline of the lake, corresponding to the entrance of a glacier valley, while the existence of one or two promontories is difficult to explain on any theory of glacial erosion.

The Val Bregaglia, again, is generally V-shaped, and he concludes that its principal features are pre-glacial, and have been little modified by the glacier. The Lake of Como is situated at the union of the Mera and the Lira, the glaciers of which have only slightly modified their valleys, and yet on their union they have been supposed to be suddenly gifted with such great excavating power as to scoop out from hard schist (micaceous and hornblendic) and granite, the open valley above the Lake of Como. On the other hand, the junction of the broad Val Telline, which must have brought down an immense affluent of ice, is supposed to have had comparatively a slight effect only. . . . A low range of hills (composed to a large extent of Nagelflue containing gneissic and granitoid pebbles, which show that the valley of the Lira was defined at a very early period) cut off

[4] Q. J. G. S. xxix. 382-401.

an arm of Lake Como from Lombardy. They form a sharp ridge with steep slopes on both sides, the top in places being almost an *arête*. . . . Here, then, in the very path of the glacier we have these ridgy hills, which the ice has but super-ficially moulded. How, then, could it excavate the lake basin? The contour also of the ground all round the head of the lake seems to be wholly uninfluenced by any great ice stream acting from the lake. From the former head of the lake near Chiavenna to its end at Lecca, is about thirty-four miles. The fall is less than 300 feet. "I doubt," says Bonney, "whether this slope would be enough for eroding a valley of this character in such hard rocks. Again, the height of Chiavenna is 1040 feet above the sea, the deepest soundings of the lake are 1341 feet, or 642 below the sea, and the height of the hills above Como is about 900 feet. To what are we to attribute an excavating power so enormous? On the other hand, ought not the rocks all round to bear most marked indications of the passage of the ice; ought not the ridges of hills above Como, which lie right in its path, to have had their sharp summits planed flat; or did the glacier when it had crawled up these steep slopes tumble, like an exhausted caterpillar, helpless down the other side?"

In regard to the lake of Osta, which is 1906 feet deep, and is also separated from the Italian plain by a low ridge of felsite, —how, if a glacier excavated the lake, did it leave intact this barrier, which it passed over, since it is strewn with blocks, moraine matter, etc.? Again, speaking of the head of the Val Mastalone under the cliffs of Monte Rosa, he says: "I was again and again struck with the absence of any relation between the principal features of the scenery and the direction of motion of the glaciers. Surely here, if anywhere, the pro-jecting buttresses of rock, which at times almost bar the valley, should have been ground away by the enormous pressure to which they have been subjected; but though rounded and worn smooth, they still remain as *barriers*, suggesting irresistibly the conclusion that they are older than, and have not been much affected by, the vast ice stream which has passed above them. . . . The singularly steep head of the Val d'Ayas is also very difficult to explain on any theory of glacier erosion. In regard to the rocks in which

the lakes lie, Mr. Bonney says no appeal can be made to their mineral structure. The Lake of Como begins in crystalline schists, and passes without much marked change in width and depth through various sedimentary rocks. The Lake of Osta lies on rocks apparently no softer than those of Val Strona, and Val Sesia. Lastly, the form of Lakes Maggiore and Lugano seem wholly inexplicable on any theory of glacial erosion, and he finally concludes that irregular movements of upheaval and subsidence, along lines athwart the valleys, explain all the features of these lakes, which "appear to be much older than the ice age, and to have been but little modified during the period of maximum extension of the glaciers." [5]

We will now turn to Professor Judd, who has been an active opponent of the theory of ice erosion of lakes. He says very rightly that the burden of proof, if it be true, rests upon those who affirm it, and he proceeds to show very conclusively that some of the principal depressions in the world, such as Lake Balaton, Lough Neagh, the great African lakes, the Dead Sea, and Jordan valley, were not, and could not, have been formed by glaciers.

The same applies to the Lakes of Van, Urumiyah, etc., south of the Caucasus, those of Nicaragua, Managua, Maracaybo, and Titicaca in Equatorial America, all of which, says Professor Judd, are lakes which no one would dream of having been formed by glaciers. In regard to the abundance of lakes in glaciated districts, he says some are in reality formed through the arrest of drainage by the peculiar and often seemingly capricious modes of accumulation of moraine matter, while others are due to local changes of level caused by differential subterranean movements along lines of drainage. Most of these, he points out, are disguised and hidden by the action of denuding causes. We can only detect on the surface a small number of the faults that actually exist. This is shown when we compare the faults marked in maps of areas where there is a rapid alternation of their beds, as in the carboniferous and oolite strata with those of limestone, lias, clay, or chalk. Even where no fractures of the strata occur, undulations and foldings of various degrees of curvature bear

[5] Q. J. G. S. xxx. 479-489.

witness to the continual action of subterranean forces . . . and for the conversion of an ordinary river valley in part of its course into a lake basin, the production of a fault is not necessary. Anyone who will examine the longitudinal section of a lake basin *accurately drawn to scale,* must admit that an almost imperceptible curvature of the strata to the extent of two or three degrees only would suffice to produce the deepest known lakes." After quoting Lough Neagh as an example where accurate mapping showed faults to exist, he says: " The amount of vertical movement necessary to produce even the deepest known lake basins bears so small a proportion to the length of the valleys in which they lie, that we do not hesitate to affirm that their effects upon the subjacent strata could not, save under exceptionally favourable conditions, be detected by the most experienced geological surveyors." [6]

Favre argues that the lakes are a direct consequence of the formation of the mountains, both owing their origin to movements of the earth's crust, and that glaciers have merely fashioned their borders. Taking the median line of the old Rhone glacier as tested by the situation of the débris, he says the Lake of Geneva has not been hollowed out in the same direction, that is, from the mouth of the Rhone to Chasseron, but in a curve bearing no relation to that line. The bend follows that of the mountains on the south bank, and the depth is related to the height of the mountains. Thus near Meillerie, where the mountains are high and the strata vertical, the lake attains its greatest depth, 265-300 metres; this, although the rocks are of hard limestone. Further west, where the lake covers soft mollasse, its depth is only 30 to 40 metres. He asks why there is no lake in the valley of the Arve, in the valley of Chamounix and the Val d'Aosta, where the glaciers must have remained longer than lower down near Geneva. He examines in some detail the position of the Swiss lakes in regard to the bends of the strata, their reversal, and their faults, and *inter alia* mentions how they often lie at the junction of beds of mollasse with hard calcareous strata; and he concludes that the position of the Alpine lakes, especially that of Geneva, has been deter-

[6] Geol. Mag., new ser. iii. 5-15.

mined along a line of overthrow or reversal of the strata. On the eastern part of that lake its form was caused by the curvature of the mountains on its south bank, and on its western part by its parallelism with the great anticlinal axis which traverses Switzerland. Its greatest depth lies along the line of reversed strata which occurs at the junction of the Alps with the plain, and the basin is therefore due not to forces acting on the surface, but to a volcanic effect.[7]

M. Falsan, speaking of the Lake of Geneva, says that it was after the Rhone glacier had turned the rocky elbow of Martigny, and been partially strangled at St. Maurice that it is supposed to have exercised its chief power in excavating. He asks if this is credible. Again, at Bouveret, the glacier turned suddenly to the south-west, and must, consequently, have lost much of its erosive power; yet it is at this point on the convex side of the lake, against which the icy current must have pressed the least, that we find the greatest depths, namely between La Meillerie and Evian; while on the concave bank, where the rocks are softer, the lake is less deep.

Again, how did the Rhone glacier spare the soft Miocene beds of the Canton de Vaud, while it scooped out the hard limestones of La Meillerie? How, again, was it that the same Rhone glacier, when reinforced by that of the Rhine and its affluents, could only erode the Lake of Neuchatel to the depth of 144 metres, and that of Morat to the depth of 52 metres; while the Rhone glacier alone is supposed to have excavated the Lake of Geneva to the depth of 334 metres.[8]

M. Falsan says, with reason, " Nous trouvons plus simple de croire que la glace n'a eu qu'á se mouler sur d'anciens accidents orographiques qu'elle a modifiés plus ou moins pro- fondément ; car, on ne peut le nier, la puissance des forces orogéniques et souterraines de la terre l'emporte infiniment sur celle des plus grands glaciers ! " [9]

In a paper on the Origin of Great Alpine Lakes, by Professor Sacco, in the Proceedings of the Royal Society of Edinburgh for 1887, in which he subjects the question to a critical examination, he concludes that the great basins of the Alps owe their origin partly to foldings and fractures of the strata, partly to subsidences and elevation. They were only

[7] Phil. Mag. 4th ser. xxix. 205-215.   [8] Op. cit. 153, 154.   [9] Id. 154.

modified to a slight degree by morainic obstructions, and by fluviatile and glacial erosion.[1]

Let us now shortly turn to the American lakes. In regard to Lake Ontario, Mr. Spencer says: "The basin is an old land valley traversed by a river, and if its waters were withdrawn its present basin would be a broad valley, continuous with that of the St. Lawrence valley, having a breadth of 30 or 40 miles.

At its western end there is an old channel revealed by borings. This is the continuation of the *cañon* of the Dundas valley, which is about two and a half miles wide, bounded by rocky walls 500 feet high. Down this valley the waters of the ancient Erie basin once flowed. It was, in fact, the pre-glacial outlet of Lake Erie. Buried channels occur at both ends of the lake, which is itself on a low flat plain now rarely submerged to a depth of more than 84 feet, which the author had shown in 1881 was drained by the Grand River and Dundas valleys into the western end of Lake Ontario. Lake Huron consists of two parts ; the southern one is a plain traversed by valleys to form only a shallow lake. Stretching for ninety miles obliquely across the lake, but not quite across, is a submerged escarpment 300 to 450 feet in height. At one point the lake is 750 feet deep. More than one buried channel is connected with the lake, forming doubtless its original drainage. "The channels," says Mr. Spencer, " at the foot of escarpments, submerged or otherwise, in Lake Huron and Georgian Bay, are fragmentary records of the history of the lake valleys." Later on, he shows how these channels once connected Lakes Huron and Ontario. In regard to Lake Michigan he says, while the deepest sounding at the modern outlet of the lake is nearly 252 feet, there are adjacent channels buried to unknown depths. Into the shallower portion of the lake the fiord of Grand Traverse Bay has a northerly trend. It is 612 feet deep. This and the lesser fiords indicate the existence somewhere of a deep channel connecting with the Huron basin, as much as the river valleys buried beneath the drift materials of the modern floor of Lake Erie prove deep channels throughout that basin, although not shown by the soundings. The southern basin of Lake Michigan is separated from the northern by a plateau

[1] Op. cit. xiv. 280.

submerged to a depth of from 300 to 342 feet, while the southern basin is now 576 feet deep. The area of this portion is now much smaller than the pre-Pleistocene valley, as its margins have been filled with drift, and now form broad plains bounding the lake. Beneath these deposits is a deeply buried channel, leading to the valley of the Huron.

"We have found," says Mr. Spencer, "one continuous channel from Lake Michigan through Lake Huron and Georgian Bay, and thence buried beneath drift deposits until it is again recognizable throughout nearly the whole length of Lake Ontario, being joined at the western portion by an ancient outlet of the Erie valley." After describing some other buried channels, he continues : "They all indicate the origin of the basins of the lakes as the valleys of a great river and its tributaries, a river of such high antiquity that the rains and rills had already ground off the surrounding hills to broaden the valleys."

In regard to the notion that these lakes were carved out by ice, he says, "Nowhere about the lakes is the glaciation parallel to the shores or vertical escarpments which are associated with the lakes. Nor are these great walls of limestone polished by an agent moving along their faces. That there are no striæ parallel to some local inlet or valley, would be perhaps rash to assert; but if so, it is a mere coincidence, with no bearing upon the origin or moulding of the great lake valleys."

The only objection to the view here maintained is frankly met by Mr. Spencer, in what seems to me a most conclusive way. He shows that certain rocky barriers, which no doubt exist, are due to quite recent warpings of the earth's crust. These are evidenced *inter alia* by the very remarkable rise in level of the former beaches. He concludes that the valleys of the great lakes here referred to, are the result of the erosion of the land surfaces by the ancient St. Lawrence River and its tributaries, and that the closing of certain portions of the old Laurentian valley into water basins occurred during and particularly at the close of the Pleistocene period, owing, in fact, to drift filling some portions of the original valley, but more especially to warpings of the earth's crust, which to a sufficient degree are measurable. In the

discussion which followed, Mr. Bonney, in supporting Mr. Spencer, said he could not understand the formation of Georgian Bay by ice, and the preservation of Manatoulin Island. His views were also supported by Mr. Irving, Mr. C. Reid, Mr. Hill, and, except as to the valley having been excavated by the rivers, by Professor Seeley ; and it seems to me that his remarkable memoir gives a final quietus to the notion that the great North American lakes were the result of erosion by ice.

The fact mentioned by Mr. Bonney in the discussion just mentioned, has been referred to in other localities as a serious difficulty in the way of the theory of the ice erosion of lake basins, namely, the existence of islands in the midst of certain lakes, *ex gr.* Loch Lomond, Derwent Water, Windermere, etc.

Lastly, we must devote a few words to the theory of glacier erosion supported by Mortillet and Gastaldi. In their view the mountain lakes were once filled with gravel, etc., and the action of the glaciers was limited to scooping out these soft contents. This view was first published by Mortillet in 1859, and subsequently developed in 1883. He quotes an observation of M. de Billy on the glacier of Görner, which in entering the valley of Zermatt travelled through the soil like a gigantic ploughshare. This must have been a very local phenomenon, since Charpentier mentioned long ago that when a glacier gets on fairly level ground, its tendency is to move over the soft beds without disturbing them. Again, as M. Favre says, either the glacier was in contact with its rocky bed, in which case it must have moved in front of it a most portentous moraine, if it pushed all the contents of a lake like that of Geneva before it, and of this we have no evidence ; or, on the other hand, if the glacier could travel over the soft cushion lying between itself and its rocky bed, it is hard to see how it could excavate even a lake full of soft materials, for the erosive force of the ice would be dissipated in the movement of the pebbles over each other, and it would no more excavate the gravel, etc., than a barrel moving on rollers would crush the ground in travelling. Besides, if the lakes were thus scooped out the gravel, etc., what came out of them must be somewhere ; where is it ?

How, again, are we to explain why the Rhone glacier, having

scooped out the Lake of Geneva, should have suddenly lost its digging power, and continued its march south of Geneva without disturbing the arrangement of the soft beds over which it then travelled ?　As a matter of fact the *scooping* out of soft materials from a hollow basin, offers almost as many difficulties as the erosion of the whole basin out of the solid matrix.

I would lastly refer to an argument employed by the late Professor Duncan that the theory of the glacial erosion of lakes ignores the biological evidence.　The living fauna and flora of the Swiss and other lakes, and notably of those of America, are quite inconsistent with their having been once filled with solid ice (which has scooped them out) and been since occupied with their present plants and animals.

In the foregoing long chapter I have tried to subject the theory of ice erosion as applied to the formation of valleys, cirques, fiords, and lakes to the careful dissection which its advocacy by so many distinguished geologists demands.　It seems to me, when so examined, to entirely break down.　Ice merely finishes the work of the erratic subterranean Pluto, or the unceasing subaerial Poseidon, to which the general contour of the surface must be attributed.　It polishes, striates, and adds the veneer or polish to the surface, but its action as an erosive agent is merely superficial.　Like the sand-paper employed by the cabinet-maker, or the burnisher applied by the sculptor, it merely finishes the surface ; and so far as we can judge, it is quite incapable of the enormous erosive action attributed to it by many modern geologists.

CHAPTER XV.

THE THEORY OF ICE-SHEETS TESTED BY THE KNOWN PROPERTIES AND HANDIWORK OF ICE.

"Nous ne devons jamais oublier que la plupart des théories les mieux établies aujourd'hui dans les sciences physiques n'auraient peut-être jamais été découvertes sans le secours des hypothèses qui ont été plus ou moins accreditées avant elles, et dont elles ont fait reconnaitre la fausseté."—BROCHART DE VILLIERS, Annales de Chimie, x. 264.

The limited areas where drift phenomena occur—Climate of Greenland and Spitzbergen probably more temperate in Glacial times than now—Nordenskiold—Difficulty of accounting for supposed ice-sheets in the districts where they are placed—Ice moving as a solid mass cannot transmit more than a certain pressure without crushing—The viscous flow of ice very slight—Its amount dependent either on the slope of its bed or of its surface—Hydrostatical movements of liquids different from those of ice—Viscous flow of ice rapidly exhausted as we near the bottom layers—In order to move over great distances on level plains, its slope and its depth at the culminating point must have been very great—Hopkins, Croll, Shaler, Agassiz—Such masses of ice would crush their lower layers—Thompson—Unweathered crests of the Alps, etc., prove they were never covered by such ice-sheets—If they had been, there would have been no supply of stones to form boulders and erratics—Where we can examine ice resting on level surfaces it does not move—Reclus, Dall—The case of Greenland—Payer, Rink, Whymper, Jensen, etc.—Nordenskiold, the Duke of Argyll, Helland—The case of the Antarctic continent—Sir J. Ross, McCormick, Searles-Wood, Wallace, Nordenskiold—Erratics on Southern icebergs show there must be exposed mountain-tops on the Antarctic continent—Lyell, Darwin, Hayes, etc.—Croll's deductions about the Southern ice-sheet untenable—If ice cannot move on level plains, much less can it move up hill for any great distance—Possibility of divergent currents in an ice-sheet—Ramsay and Goodchild—Difficulty of explaining the lifting up of boulders from lower to higher levels by ice-sheets—Criticism of Wright's views on this subject—Moving ice does not break up its own bed—Irving and Campbell—Different kinds of moraines—All different from the Drift—M. Williams and J. Geikie—Objections to the theory of Ground moraines—Lyell, Southall, Callard, Bonney—Impossibility of an ice-sheet being at the same time padded with soft materials and an erosive agent—Bonney, M.

Williams, M. Reade, Dakyns—Mechanical difficulty of transporting deep beds of drift by ice-sheets—Young, Searles-Wood, jun.— Occurrence of marine shells in drift incompatible with ice-sheets— Lyell and Phillips—Difficulty of supposing that sea-bottoms with shells on them were scooped out and raised to great heights by ice-sheets, as urged by Croll, Trimmer, Belt, etc.—Replies of Bonney, Green, and Macintosh—Difficulty of accounting for intercalation of beds of sand, etc., among the glacial clays by ice-sheets—Similar difficulty arising from the distribution of the drift materials according to their specific gravity; from the occurrence of erratics in groups and alignments; from the so-called driftless areas; from the mixture of erratics which must have come from different directions; from the occurrence of masses of drift gravel in the heads of mountain valleys; from the huge boulders of Cromer, etc.; and from the occurrence of striæ on the crests of hills where the ice-sheet could not secure stones to act as eroding chisels.

WE have now reached the most critical part of our subject. In the preceding chapters I have been urging views which would probably be accepted by a considerable number of geologists, although they are contrary to the conclusions of an extreme and popular school of glacialists. I have now to press my conclusions to what I deem to be a logical issue, and in doing so shall have to traverse arguments which are much more widely accepted, and which pervade most geological text-books.

We have reached the point, that there is no good warrant for believing in more than one glacial period, and that this one was much more limited in its operations than has generally been supposed. The evidence for a glacial period in the southern hemisphere completely breaks down, and instead of it, we merely have established the existence of a number of local glaciers on a much greater scale than any now remaining. In the northern hemisphere we have found that the glacial phenomena are wholly wanting in one half the circumpolar region, and virtually no traces of them can be found in the vast district from the White Sea eastward to the River Mackenzie. This important fact is admitted and tabulated in most recent maps showing the distribution of the drift. If we now limit ourselves to that portion of the northern hemisphere where the drift is generally conceded to prevail, viz., from the Mackenzie River eastward as far as the White Sea, we shall still have to exclude a considerable area if we do justice to the facts.

Over a very great portion of this district there is no

evidence whatever of glacial conditions having prevailed at all, namely, over that part of it covered by the North Atlantic; nay, more, we have only slight evidence, so far as I know, that the so-called glacial age extended to Greenland. It may have done so, but we have little positive evidence. If we are to judge of Greenland during the period about which we have records, it seems clear that while it has been rising from the sea it has also been growing colder. The glaciers have greatly increased, and the ice has encroached upon the old dwellings and the old hunting grounds of the Esquimaux, which are no longer habitable in many places, while it would seem that during the domination of the Norsemen the climatic conditions of the country were more temperate.

Nordenskiold, referring to the shells in the clay beds at Sarpiursak, in Greenland, says : " These shells and remains of fish had been noticed by the Greenlanders. They are mentioned by Dr. Rink in his work on North Greenland, who adds that a collection was sent home and examined by Dr. O. A. L. Mörch, who found the shells to belong partly to species still living on the coasts of North Greenland, partly to more southern forms." [1]

Speaking of the Jacobshavn fiord, the same writer says : " This fiord is found inserted on very early maps of Greenland, though generally as a sound uniting the North Atlantic with Baffin's Bay. It is now known that the supposed sound is only a deep fiord filled throughout its length with huge icebergs. A tradition exists among the Greenlanders that it was in former times less obstructed by ice, and was consequently a good hunting and fishing place, and this is confirmed by the older maps of the fiord, but especially by the numerous remains of old dwellings which are still met with along the shores, not only of the principal fiord, but of its southern arm, Tessiursak, now completely barricaded by icebergs and inaccessible from the sea." [2]

" The walrus no longer ventures into this long ice-blockaded fiord, and even the bear has now become so scarce in the colonies of North Greenland south of the Waigat, that most of the Danes resident there have not seen it. The remnants of bones in the kitchen middens prove, on the other hand, that

[1] Nordenskiold, Geol. Mag. ix. 409-30.   [2] Geol. Mag. ix. 412.

these animals were once abundant here, and are consequently an evidence that the fiord of Jacobshavn was less filled with ice than now. . . . And that accordingly the same phenomenon, though on a larger scale, has taken place here as in the northern harbour of Belsound, Spitzbergen. Krantz mentiones a similar case with reference to the ice fiord, north of Frederickshaaf in South Greenland."[3]

It seems, further, clear from the banks of shells found by Nordenskiold in Spitzbergen, which date apparently from Pleistocene times, and which comprise molluscs no longer living so far north, that we may extend our arguments to Spitzbergen, and exclude it also from the extreme glacial conditions which are supposed to have been present elsewhere in the northern hemisphere at that date.

Excluding this district, we are limited, in fact, to two wide areas where the so-called glacial phenomena are virtually omnipresent. One in North-western Europe, and the other in North-eastern America. Each of these areas was occupied, according to the chief champions of a great ice age, by one or more ice sheets : in America by one culminating somewhere in Labrador, and spreading out in all directions; in Europe by several, the chief one culminating in Scandinavia and Finland, and others in the Alps and lesser ranges of Western Europe, and similarly spreading out in all directions. In regard to this view, it is puzzling and difficult to explain how such ice sheets could be formed at all in these situations. What could induce an abnormal snowfall, or favour an exceptional accumulation of ice in these particular limited districts ?

It is possible to realize a physical cause and an explanation of a great accumulation of ice over the Polar area culminating near the Pole, or that a similar mound of ice might have existed in the district surrounding each of the two poles of cold ; or again, if there had formerly been some very high masses of high land forming a focus to such a gathering of ice and snow in these areas ; but none of these conditions are present there. Why, then, should there have been a vast accumulation of ice over the asymmetrical districts where the glacial phenomena prevail in Europe or in America ? What,

[3] Id. 415-16.

again, could induce a vast accumulation of continental ice in one half the circumpolar region when the rest of that region was enjoying temperate conditions ? These are, I confess, very great puzzles, and I have never seen any reasonable explanation of them. Yet, if such ice sheets did exist as have been postulated by Croll and Geikie and others—ice sheets which could traverse the deep basin of the Baltic and march over 600 miles to the south of it, which would fill the North Sea and overwhelm Great Britain in their folds, and which could carry the rocks of the Laurentian highlands seven hundred miles away over the prairies—their depth and size must undoubtedly have been portentous, and must have had some very definite cause. It seems to me that it is not an unreasonable demand when we ask its advocates for an explanation of this accumulation of ice under such conditions in these particular districts, as a preliminary step to our entering into the details of the theory.

Let this pass, however. The ice when formed is supposed to have moved out in all directions and distributed the boulders, clay, and other drift products over expanses of level country, stretching in some cases for hundreds of miles. It has been taken for granted that this actually occurred, and no attempt, so far as I know, has been made to show how it could happen consistently with the known properties of ice, and especially with the fact now established that ice can only move under the influence of gravity. We saw in the last chapter that, when moving in a solid mass, a glacier cannot advance more than a short distance from the slope which gives it its birth, since the necessary pressure to make it move cannot be transmitted through it for a great distance without crushing it. This infirmity of ice applies of course to it when massed in ice sheets just as much as to glaciers. Nor is the question one of much interest to us, because it is clear that when ice moves as a solid mass and entirely in consequence of its weight, it must have a slope to give it impulse. On a level surface the weight of such an ice-mass can only press in one direction, namely, vertically.

If ice is to move by virtue of gravity over a level surface, its motion must be differential, and due to its viscous nature. Now while it is true that ice has been shown to be viscous,

and that the main element in glacier motion is a viscous motion, this viscosity is very limited compared with that of other viscous substances.

It requires a long time and great pressure to cause it to move, and it has a very strong inherent stability of its own. If it were not so, it would be impossible to find it standing up in cliffs hundreds of miles in extent and hundreds of feet high, like those observed in the Antarctic regions and like the cliffs on each side of crevasses, or like similar perpendicular ice-faces in Greenland. Nor would huge icebergs continue to stand out of the water with abrupt scarps all round them. They would inevitably flow outwards and form sloping sides. We are not surprised, therefore, to find that while a glacier (when on a sufficiently sloping bed and urged on by a sufficient thrust caused by its great weight) will move as a viscous body, it does so only slowly, and that its motion is very largely a function of the slope on which it lies, and that in some cases it can maintain itself on slopes of a considerable angle without moving.

From his experiments on the small glacier of the Schonhorn in 1844, Forbes deduced the important conclusions *that, other things being equal,* the velocity of a glacier is proportional to some function of the declivity upon which it rests. In these experiments, the velocity under a slope of 20° was about a third greater than under a slope of 10°; but as in the case of a river, the slope is only one of several considerations, as : I. The mass of the viscous body; the smaller the mass the smaller the velocity on a given slope. II. The state of infiltration or wetness of the glacier, altering its resistance to change of form.[4]

This reasoning seems further confirmed by observations made on the Moteratsch glacier by Tyndall and others. Some distance up the glacier, the movement at its maximum was found to be fourteen inches per diem; yet at the snout, which lies on a nearly horizontal bed without ice in front to offer any resistance to its motion, the movement forward was only two inches in a day.[5]

On another occasion Tyndall called attention to the fact that the medial moraine of the Morteratsch glacier widens

[4] Phil. Trans. 1846.　　　　　[5] Q. J. G. S. xxxix. 66.

as we descend it, from a mere strip of débris at its upper part until it overspreads the entire glacier towards its end. He measured the rate of motion at three different points and showed that at the highest point it moved 56 inches in 100 hours, at the next 47 inches, and at the third 30 inches; proving that the snout of the glacier moves much slower than the upper part, hence the gradual crowding of the stones.[6]

It is clear, therefore, that although ice be viscous, its viscosity is so slight that it requires a very considerable impetus from gravity to make it flow. In a glacier it derives this impetus from the fact of its resting on a sloping bed; so far as I know, it is only when on slopes or close by them that glaciers have been seen or known to flow at all. This is why, in ordinary glaciers, as has been remarked, the ice is thin at the commencement, and thickens as it descends into the lower valleys, where the slope is less and the resistance to motion is greater.

The Duke of Argyll, arguing from this fact, says, "I cannot help thinking that there is a fundamental fallacy in comparing the movement of ice masses down the slopes of a mountain with any movement of such masses which is possible on level ground or against opposing slopes. In the one case gravity is an assisting, in the other case it is a resisting force. In the one case the heavier the mass of ice the easier and faster will be its motion; in the other case every additional ton must add to the difficulty of effecting movement. In the one case thrust and gravity act together; in the other case thrust must act alone with gravity and friction to counteract it. And then it must be remembered that in the case of mountain glaciers, the thrust itself is due to superincumbent weight, being the result of the accumulation of snow on the mountain tops; whereas it does not seem clear how superincumbent weight upon level ground, and at a distance from mountains, could have any effect in producing horizontal motion, and if such effect were produced, it would probably be expended in a slipping of the ice over itself, rather than in any progress of its base over rough and ascending surfaces of rock."[7]

If this be the *raison d'être* of the motion of modern glaciers, it is clear that if we are to explain the phenomena of

the drift by means of land ice, we must either postulate that the land in certain districts was elevated to an enormous height above its present level so as to give a sufficient slope to the ground to drive the ice for hundreds of miles from its source, or the postulated ice sheets must have been very different in nature from modern glaciers. No one that I know of has been brave enough to face the former alternative, and the champions of ultra-glacial views cheerfully accept the latter one.

Thus, Mr. J. Geikie scouts an appeal to glacier action as we know it in the Alps, Norway, etc., as inconsequent, and bids us remember that a mountain-valley glacier is one thing, a glacier extending far into the low grounds beyond the mountains, and it may be coalescing with similar ice flows, is another and very different thing.

Holding this view, which is in fact a necessity of their position, the advocates of ice sheets have appealed to an entirely different impulse from that which caused a glacier to move. They have argued that an ice sheet will move independently of the slope of its bed, and cause a lateral thrust upon objects beneath it, *if its surface differs in level in different places.*

If this be so, it must be in one of two ways, either by hydrostatic pressure, or by the ice rolling over itself like grain poured out on a floor. Let us first consider the former method.

No doubt a liquid whose surface is not level will adjust itself to a condition of equilibrium by sinking where it is highest and rising where it is lowest, and in doing so will exercise a lateral thrust upon any objects lying below it. The measure of this thrust will be the pressure of a vertical column equal in height to the difference in level of the parts of the liquid under consideration. This is quite true of a liquid; but, as Forbes long ago urged, " if the body is only semifluid, the consistency or mutual support of the parts opposes a certain resistance to the pressure, and prevents its indefinite transmission." The more rigid the substance is, the greater will be the interference due to internal friction, and if the viscosity is very great, this friction will speedily counterbalance every tendency to hydrostatic movement. We can

measure the relative natures of water and ice in this behalf. In the case of a glacier whose top and bottom differ in level by 4000 feet, if the ice were converted into water its lower end would begin to move with the enormous velocity of 506 feet a second, or would move over 44,000,000 of feet in twenty-four hours. Now the Mer de Glace, the largest of Alpine glaciers, moves only about two feet in the same time.

This shows what a vast difference there is between the viscosity of ice and water. In regard to hydrostatic impulse, I do not believe it possible for ice to move in this way at all, the tremendous molecular friction involved in any hydrostatic movement, forming quite an inseparable barrier to its doing so ; and, so far as experiment can guide us, it is uniformly consistent in support of this view.

Nowhere in nature do we see ice rising and swelling in one place and sinking in another, by ordinary hydrostatic pressure, as we should in the case of a liquid. The lower layers of ice walls and cliffs in crevasses and on the sides of tabular glaciers are not seen to bulge out in the way required by the theory. I therefore conclude that the notion of ice moving by ordinary hydrostatic methods fails.

We are consequently remitted to the second form of molecular motion, namely that under the influence of which ice spreads itself out like a heap of grain poured out on a floor, or a mass of treacle or pitch on a table.

Ice acting in this way has, I take it, a double motion. In the main it moves by simple gravity, due to the efforts of each particle of ice to roll down upon the particles below it, until a stable level is produced, which tendency is opposed by the friction and the cohesion between the particles. We can measure its amount by experimentally verifying the shearing force of ice, and also, in the case of glaciers, by discovering at what slope the motion commences. We know that it is in fact very slight. In addition there is a dragging tendency by each layer of the ice upon the layer below it.

Let us first consider the former case. In order that ice resting on a flat plain should begin to roll over itself as a viscous body does, its upper surface must have a slope at least equal to that of the bed of a glacier of similar weight when it first induces molecular movement in it ; and inasmuch as this

slope must be continuous over hundreds of miles if the ice is to move over hundreds of miles, as it is supposed to have done in Poland and North America, we get a measure of the immense depth which the postulated ice sheets must have reached in their culminating domes.

Hopkins found that ice in laboratory experiments barely moves on a slope of one degree. Taking this as a basis, Croll applied it to test the thickness of the supposed Antarctic ice sheet. The Antarctic continent is about 2800 miles across, so that its semi-diameter is about 1400 miles ; its edge, he calculates, is about 1400 feet thick, and its rate of progress about a quarter of a mile a year. From these elements, he concludes that the ice at the Pole must be twenty-four miles thick.[8] Professor Shaler says: " To give such a slope as would not only move the glacial sheet over ten degrees of longitude on level ground, but up a long slope, would be prodigious, for only a slope of over one degree from the shores of Hudson's Bay to New York would require a thickness of many miles at the source of the stream.[9]

If we adopt another measure, namely the smallest slope on which a glacier has been supposed to move, i.e. 1 in 211, we still should require a depth of seven miles at the Pole if we are to explain the movement of an ice sheet such as that which is supposed to cover the Antarctic region. If we again take the measure adopted by Agassiz, we have a similar result. He argues that " in order to advance across a hilly country and over mountainous ridges, rising to a height of twelve or fifteen hundred feet in the southern part of Maine, and to a much higher level in its northern part, the ice must have been *several times thicker* than the height of the inequalities over which it passed; otherwise it would have become encased between these elevations, which would have acted as walls to enclose it. We are therefore justified in supposing that the ice-fields, when they poured from the North over New England to the sea, had a thickness of at least five or six thousand feet." [1]

Professor Dana estimates the ice in the northern part of the United States to have had a thickness of from 5000 to 6500 feet, and in the south an average of 2700 feet. The pressure,

[8] Discussions, etc. 244-45.   [9] Geol. Mag. 1882, 36.   [1] Id. lxi.

he says, must have been immense, 6000 feet corresponding to at least 300,000 lbs. to the square foot.[2] Six miles thick of ice really gives a weight of 12,000 lbs. to the square inch, or 21,000 tons on a square mile, or a pressure of 800 atmospheres.[3]

These portentous figures are easily invoked, but it is forgotten that under any such pressure, as Sir Wm. Thompson and others have shown, ice would crush and liquefy. This shows how transcendental the appeal becomes when tested by figures.

Not only so, but we know as a matter of fact that both in the Alps and the Dovrefelds the higher parts of the mountains retain their rugged, weather-eaten contours and surfaces, showing that they were never completely covered with ice, as the conditions demanded by Croll and others require.

So far we have merely considered the movement of the surface and superficial layers of ice and the angular blocks which the ice sheet may have carried on its back; but in order to spread out the drift as we find it, not only the surface layers but those at the bottom of the ice must have had a considerable movement so as to roll the stones and also to distribute them. This motion of the lower layers of the ice sheet could only be produced on level ground, it seems to me, by the dragging and pulling tendency of the upper layers.

When on a slope, and when the motion of the ice is comparatively considerable, the residual drag upon the bottom layer of ice which remains after the full amount of gravitating power has dissipated itself in the movement of one layer over another is scarcely an appreciable quantity; but when we get on to level ground and we have nothing to help us save the amount of surface slope of the ice itself, this residual force must be virtually reduced to nothing.

This must be so, unless we can secure a very steep slope for the surface of the ice sheet, and one continuing over a long distance; but how are we to get such a slope originally, and having got it, how are we to maintain it? No doubt ice, like other viscous bodies, will begin to roll over itself if its slope be sufficient to enable gravity to overcome its internal friction;

[2] Geol. Mag. x. 277.   [3] Geol. Mag. 1882, 403.

but directly the slope is reduced to an angle of equilibrium the slipping ceases and this kind of motion becomes impossible.

Again, if we are to secure sufficient motion *to make itself felt at the base of the ice sheet,* we must have *a very considerable* sudden or rapid addition of ice to the upper part of moving mass; but this could not be secured, it seems to me, for there would be a continual adjustment going on. The additional height gained by every snowfall would begin to be dissipated as soon as it had fallen, and such additions would not do more than give a certain motion to the upper ice, and would and could not reach the lower layers.

We know that in the case of glaciers moving with all the advantages which gravity gives them on a sloping bed, the motion which we can measure in the upper layers is virtually reduced to zero at the bottom; *a fortiori* must this be the case on a flat surface.

It seems to me quite plain that in postulating the portage of the drift by means of ice sheets, glacial geologists have forgotten to establish the physical possibility of the process they argue from, which ought to be the initiatory step in their argument. Suppose, however, the motive force were available, how are the materials of the drift to be supplied? As we shall see presently, the rounded stones which are found in moraines, and have been rolled by the ice and the water beneath the glaciers, have all come originally from its surface, on to which they have dropped from overhanging rocks. They are merely angular blocks which have fallen down crevasses and been rolled and rubbed into boulders beneath the glacier; but if the ice were piled up to the extent required there would be no bare rocks to supply the boulders. This presents us with another very serious difficulty in regard to continental ice sheets.

How is it possible to secure such a surface slope as would move ice from the Laurentian highlands, which are not very high, 600 miles away; to move Norwegian ice to Norfolk and the Shetlands, and Finnish and Swedish ice to Central Russia and the Carpathians, without at the same time so completely burying the whole country in ice as to prevent any boulders from falling upon it from exposed rocks? I do not see how

this is to be explained. We actually find in Greenland that there are no moraines, except where the so-called nunataks or exposed rocks project above the ice ; and in the districts where it is unbroken and continuous, like those traversed by Nansen, there are no moraines at all. *A fortiori* must this have been the case when, as it is argued, the whole country was swathed and buried miles deep in ice.

Not only so, but in the case of ice moving over level ground, there would be no crevasses by which the surface blocks could be dropped into the glacier mill. Crevasses are the gaps and fissures caused by the strain which a varying slope in the bed of a glacier causes, and they close up when it reaches level ground. For these two good reasons, therefore, there would be no drift available for the ice-sheet to distribute, even if we could secure its acting in the postulated way.

So far we have argued the question on *a priori* grounds alone ; let us now face the problem in its concrete form. First, so far as we know, masses of ice on level ground are always stationary.

Reclus says : " Even the plains of more desolate regions, which stretch away to the north of the continent of Asia, have glacier-like masses in which nothing is wanting but motion to make them resemble those of the Alps. The snow, driven by the eddies of the wind, is heaped up in the hollows of the ground so as to form complete hillocks, which the heat cannot entirely thaw during the short days of summer, and after the middle of autumn these heaps begin to increase. In consequence of the partial melting and the successive freezings, the snow composing these hillocks is changed first into *névé* and then into ice, pure and blue, like that of the Alps. The mass presents some clefts, caused doubtless by the sudden change of temperature, *but it does not shift its position on the surface of the ground* as glaciers do ; only the thaw water produced by the sun on the surface of the mound of snow, flows down its sides, and then freezes again, thus giving a wider base to the hillock. Many of these patches of ice, which on sloping ground would serve as the beginning of a glacier, are some hundreds of yards in length." [4]

Dall speaks of a ridge of solid ice in Alaska, rising several

[4] Reclus, The Earth, 242.

hundred feet above the sea, and higher than any of the land about it, and says, " This is covered with soil and vegetation. The continuity of the mossy surface," he adds, " shows that the ice must be quite destitute of motion." [5]

Croll, Geikie, and others apparently contemn such instances, and continually bid us turn, if we are to understand the ice age, from the puny glaciers of Europe and America to Greenland and the Antarctic continent, where undoubtedly the conditions are on a much greater scale. While the 60 primary glaciers and the 1000 secondary ones in the Alps cover a superficies of only 55 or 60 square miles, and the glaciers of Justedal and Folfegon in Norway are respectively only 15, 16, and 5 square miles in extent, the continental ice of Greenland is probably 2000 square miles in extent. [6]

That the Antarctic and Greenland ice form vast accumulations we all admit ; what we do not admit is that this ice moves in some extraordinary way independent of gravity, and quite differently from ordinary glaciers. In order to sustain this conclusion, Croll, Geikie, and others have argued that both Greenland and the Antarctic continent are, in fact, composed of low-lying lands off which the ice moves, as their postulated ice-sheets are supposed to have moved in the Ice Age, by some transcendental and inherent force apart from gravity.

It is, therefore, important to inquire what the contour of these districts really is. First, in regard to Greenland.

Payer, who accompanied the second North German expedition in 1869 and 1870, speaking of the coast between lat. 73 and lat. 77, reports it as much broken, and the glaciers as subordinated in position to the higher peaks, and having their moraines both lateral and terminal like those of the Alpine ranges, and on a still grander scale. Petermann Peak in lat. 73 is reported as 13,000 feet high. Captain Koldewey, chief of the expedition, found extensive plateaux on the mainland in lat. 75 to be " entirely clear of snow although only sparsely covered with vegetation." The mountains in this vicinity also, rising to a height of more than 2000 feet, were free from snow in the summer. Again, when the crew of the *Hansa* floated down the coast on an iceberg, Dr. Wright

[5] Amer. Journ. of Sc. cxxi. 1888, 106–109.
[6] Meddel. om Grönland, etc., i. 181.

says: "As viewed from this unique position, the coast had the appearance everywhere of being precipitous, with mountains of considerable height rising in the background, from which numerous small glaciers descended to the sea-level." [7]

Rink, speaking of Greenland, says: "Towards the interior the inland ice is bordered by a row of nunataks (i.e., mountain tops projecting above the ice, so named by Nordenskiold) distant about forty miles from the seaward edge. . . . The nunataks had been an obstacle to the movement of the ice, which on the east side facing the interior was broken and piled up against the rock for several hundred feet, while to the south and north, and between the nunataks, it poured down like frozen waterfalls." [8]

Whymper says: "The whole of the (outer) land on the (west) coast of Greenland is mountainous, and although the hills scarcely if ever exceed 8000 or 9000 feet in height, they effectually conceal the inner or glacier covered land." Speaking of the ice-sheet, he says, " From the nearest parts to the farthest distance that could be seen, the whole of the ice was broken up into *séracs*. It was almost everywhere riven and pierced in the most extreme way." [9] Mount Kelertinguit was 6800 feet high.[1]

In 1878, Jensen penetrated 75 to 76 kilometres upon the ice of the south of Greenland, and found it entirely surrounded by nunatakker or mountain summits projecting from the ice. The mountains there, were 1571 metres above the sea level. Mr. Komerup collected fifty-four species from the nunatakker, consisting of phanerogamous plants and mosses, proving how long they must have been bare in summer. The arrows on his map illustrating his journey show that here, at all events, the ice does not move in a direction independent of the contour of the country, but twists in and out of the nunatakker, showing how they dominate the direction of its flow. In the words of Mr. Johnstrup, "Elle serpente souvent dans des directions tout à fait opposées autour des masses rocheuses qui en émergent comme des iles." It was

[7] Ice Age in N. America, 90.
[8] Trans. Geol. Soc. of Ed. ii. 286–293.
[9] Alpine Journal, 1873, 220.   [1] Id.

noticed that the character of the crevasses and also their direction, was dominated by the shape of the bed over which the ice flowed.

In 1883 and 1885 a special expedition was sent to explore the eastern coast of Greenland, north of Anarket. In the French epitome of the report of this exploration we read that this coast is cut into by deep fiords crowned by high rugged mountains, which have never been covered by the continental ice and in some places are characterized by a relatively rich flora. Between the mountains are numerous glaciers which descend into the fiords, while in the interior of the country there extends a mountainous region filled by great local glaciers.[2]

Between Kangerujuk and Iluilek, at one mile from the coast, mountains of 942 metres high are not rare. In the middle of the district there are several of about 2200 metres in height. On climbing to a considerable height, continues the reporter, " On découvre qu'il y a un grand nombre de régions montagneuses et de chaines de montagnes d'un aspect sauvage qui s'élèvent très haut au-dessus des parties recouvertes par la glace. Ce sont donc ces régions montagneuses et non la glace qui frappent le plus dans le paysage. Celle-ci peut s'etendre sous forme de glaciers ou de neige jusqu'au sommet des plus hautes montagnes, mais elle ne couvre principalement que les régions situées entre les montagnes jusqu'au une hauteur de 3000 pieds environ." [3]

From Iluilek to Cape Adeleer, moderately high mountains with intermediate valleys reach the coast. They generally end in abrupt faces; their mean height is about 470 metres, while some are as high as 785 metres, but they rapidly rise as we travel inland, and at the heads of the fiords groups of scarped mountains occur almost everywhere of from 3000 to 6000 feet in height. The continental ice cannot traverse these obstacles, and it is only in certain places as at Cape Tordenskiold that it reaches the sea. The region, however, is rich in local glaciers.[4] . . . North of Puisertok, and in the interior of the country, numerous nunnatakker rise above the ice to a height of 5000 feet (1570 metres). . . . At six miles from the outlet of the fiord of Kangerdlugmatsiak,

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[2] Meddelelser om Grönland, ix. 370.　　　[3] Id. 371.　　　[4] Id. 372.

a height of more than 1900 metres, there rises a group of pointed mountains, whose summits are covered with snow. This group and two others almost equally imposing, are separated by profound ravines filled with currents of ice. . . . In the region north of Umanak, mountains of 5 to 6000 feet in height girdle the heads of the fiords. . . . Between Cape Moltke and Kangerdlugmak there extend mountains 3000 to 4000 feet high.[5] A great glacier, called Kangerajup apusinia, extends from Tingmiailahk to the fiord of Uriuvik. It is interrupted here and there by nunnatakker. It rises, we are told, by a steep ascent towards the snow-covered mountains forming long chains and peaked heights which lie behind it.[6] North of Uriuvik, and a little way inland, there rise some peaks which are far above the snow. The most conspicuous is that of Arukitsok, 1210 metres high. North of the fiord of Kardlit ikera, the nunnatak rises to a height of 550 feet, and is quite bare of snow on the sea face.[7] Immediately east of Inigsaleh the country changes ; the continental ice disappears from the coast and gives place to high mountains, among which are valleys and ravines with a luxurious vegetation.[8] The western side of the fiord of Sermileh is very mountainous, the heights being 3000 to 4000 feet in elevation, and at its inner recesses, on its eastern bank, there rise peaks to the height of 6000 feet. The district of Kingorsuah is surrounded by mountains more than 6000 feet high among which glaciers intertwine, though they do not reach the fiord. Speaking of the fiord of Sermiligak the reporter says : " L'intérieur du pays présente un chaos de cimes hautes de 5000 à 6000 pieds, et en partie couvertes de neige. Il y avait plusieurs glaciers entre les montagnes."[9] Again he says : " Sur l'etendue comprise entre Sermiligak et Kialinek, les montagnes sont tout aussi hautes et aussi dentelées que dans le district d'Angmasalik."[1]

Near the nunnatakker of Jensen there occur two considerable moraines consisting of several hills. One of these measures about 2767 metres in length and is about 125 metres high, and consists of gravel and sand overlying ice. The stones in this moraine, curiously enough, are all polished

[5] Id. 375. [6] Id. [7] Id. 376. [8] Id. 377.
[9] Id. 379. [1] Id. 382.

and have their angles rounded. This shows they must have come from a distance, and not from the nunnatakker close by, which are too near for the ice to have caused this rubbing down. In addition to the two large moraines there were also isolated ones, apparently unconnected with the nunnatakker. In one of these were several blocks of twenty feet in diameter. It was noticed that these blocks invariably fall down crevasses as the ice moves on to the more broken ground.[2] Thus the absence of travelled blocks in certain parts of Greenland is no evidence that there are no projecting rocks behind.

In the fiord of Umanak the ice often contains considerable masses of stone and earth, which are only disclosed when the surface melts.[3] These seem to me to point to the existence of nunnataks far inland, whose *débris* has been covered by more recent snow-falls.

Nordenskiold says : " The Greenland ice continually carries with it basalt blocks and basalt columns, indicating that the lofty inland mountains are still composed of that rock; and that it is interstratified with Tertiary schists is evidenced by the plant remains which, on the Asakak glacier, lie mixed with pieces of basalt on the surface of the ice. "[4]

It has always been remarked, as a circumstance that needs explanation, that the Greenland glaciers have a motion, both in summer and winter, much in excess of that of the smaller glaciers of the Alps, and Helland found that the Jacobshavn glacier advanced at the rate of four miles a year. This more rapid movement has been generally attributed to the greater masses of ice of which these Greenland glaciers are composed. I believe it is more largely due to the rapid slopes down which they have to flow to get into the sea.

The Duke of Argyll has pointed out that the very broken and hummocky character of the surface of the ice plain in Greenland is also due to the irregular surface on which it moves.

Helland says : " That the islands or Skärgärd on the coast of Greenland represent in miniature the whole country, except that these islands are covered up and connected by a continuous ice sheet is an idea with which anyone who is familiar with a

[2] Id. 188-9.   [3] Op. cit. iv. 267.   Geol. Mag. ix. 423-4.

fiord and its ramifications cannot agree. Greenland is a fiord land like Norway and North-West America. So that if we could form a conception of what is concealed by the ice in it, we must study what is uncovered in them. It is obvious that large, deep and strongly marked fiords do not intercept the country as sounds, but pierce into the land as valleys whose ramifications at last die away in the interior. We conclude, therefore, that the structure of Greenland is somewhat similar, and so that it is not a collection of islands." [5]

This evidence which I have purposely given in some detail, since the issue is important, points unmistakably to Greenland being a high plateau girdled with precipitous rocks, with its surface contour smothered by ice, which in certain places rises in a dome-shaped contour. This ice is not planted on a level surface, but is clearly moulded upon high lands everywhere.

The Duke of Argyll, commenting on Dr. Kane's description of the Greenland coast, says : " The shape of the hills is everywhere determining the shape of the ice streams ; and only in a minor degree, and as a secondary consequence, are the ice streams modifying the shape of the hills. Great valleys or sweeping hollows, marked by containing walls and buttresses of rock frequently piercing the perpetual snows, bound and define the glaciers towards the coast; and as far as the eye can reach to the lofty summits of the interior, the surfaces of the ice and snow give evidence of every variety of form of the underlying slopes and precipices upon which they are moulded, and over which they are slowly slipping to the sea. Moreover the ocean sounds into which they fall at last, and which they people with a stately procession of enormous icebergs, are channels lying between coasts whose general trend is at right angles to the direction of the ice streams which cut them. Those lines of coast must therefore have been determined by other causes, and the boundaries of the land and sea, which are of course inseparably connected with its elevation, and with the general contours of its surface are seen to be equally independent of the comparatively superficial work done by the snow which gathers upon its tops, and which is shed from its sides. " [6]

[5] Q. J. G. S. xxxiii. 147.   [6] Q. J. G. S. xxix., liii. and liv.

Let us now turn to the Antarctic continent, which has not been so much explored as Greenland, and which is therefore a safer region in which to speculate on possibilities and potentialities. It is the region to which Dr. Croll continually turns, and is the instance on which he largely relies to prove his case. He will have it that the Antarctic continent is not a mass of high land, but that the ice found upon it is essentially an ice sheet moving in all directions because of the tremendous depth of ice piled about the Southern pole. This opinion seems to me to be based on no reliable evidence.

" We had in the afternoon," writes Sir John Ross, in February, 1841, "a good view of the coast. The whole of the land being free from cloud or haze, the lofty range of mountains appeared projected upon the clear sky beyond them, beautifully defined; and although of a spotless white, without the smallest patch of exposed rock throughout its whole extent to relieve it, yet the irregularities of the surface, the numerous conical protuberances, and inferior eminences, and the deeply marked valleys, occasioned many varieties of light and shade that destroyed the monotonous glare of a white surface." The Duke of Argyll, commenting on this description, says, " It enables us at a glance to be certain that no ice sheet covers up and conceals the mountains of the Antarctic continent. They are indeed covered by deep snow, but by snows which are moulded by the terrestrial surfaces below them, which surfaces were determined before these snows had begun to fall, and to the minutest inequalities of which the snowy covering conforms. . . . The ice sheet which exists below is simply the confluent glaciers which are formed in the old terrestrial hollows, and whose motion is due to the steepness of the old terrestrial hills. Where these hills are of great elevation the ice sheet is deep, where they are low the ice sheet is comparatively thin. " [7]

" On the 11th of January, 1841, in about lat. 71° S. and long. 171° E.," says a graphic writer, "the Antarctic continent was first seen, the general outline of which at once indicated its volcanic character, rising steeply from the ocean in a stupendous mountain range, peak above peak enveloped in perpetual snow, and clustered together in countless groups resembling a vast

[7] Ross, Voyage, i. 257.

mass of crystallization, which, as the sun's rays were reflected
on it, exhibited a scene of such unequalled magnificence and
splendour as would baffle all power of language to portray, or
give the faintest conception of. One very remarkable peak, in
shape like a huge crystal of quartz, rose to the height of 7867
feet, another to 9096, and a third to 8444 feet above the level
of the sea. From these peaks ridges descended to the coast,
terminating abruptly in bold capes and promontories, whose
steep escarpments, affording shelter to neither ice nor snow,
alone showed the jet black lava or basalt which reposed
beneath the mantle of eternal frost." . . . "On the 28th, in lat.
31 and long. 167° 1´, the burning volcano Mount Erebus was
discovered enveloped in ice and snow from its base to its
summit, from which a dense column of black smoke towered
high above the other numerous lofty cones and crateriferous
peaks with which this extraordinary land is studded, from the
72nd to the 78th degree of latitude. Its height above the
sea is 12,367 feet, and Mount Terror, an extinct crater near to
it, which has doubtless once given vent to fires beneath,
attains an altitude little inferior, being 10,884 in height, and
ending in a cape from which a vast barrier of ice extended
in an easterly direction, checking all further progress south."[8]

Mr. Searles Wood, Junr., says, " The glimpses obtained by
Sir James C. Ross's expedition of the mountains cutting the
distant horizon behind the perpendicular ice wall, which was
found by that expedition to extend for hundreds of miles in
very deep water, lead to the belief that some of the Antarctic
land which lies beneath the vast mass of ice, whose seaward
termination forms this wall, must rise to great altitudes ;
while the two great volcanoes discovered by that expedition
in the same region, by their great altitude, point to the same
inference."[9]

" In the Antarctic regions there are extensive highlands
and lofty mountains," says Wallace, " and these are every-
where exposed to the influence of moist sea air, and it is here,
accordingly, that we find the nearest approach to a true ice
cap covering the whole circumference of the Antarctic conti-

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[8] Remarks on the Antarctic Continent and Southern Islands, by
R. McCormick.
[9] Geol. Mag. 1876, 443.

nent, and forming a girdle of ice cliffs which almost every-
where descend to the sea. Such Antarctic islands as South
Georgia, South Shetland, and Heard Island, are often said to
have perpetual snow at sea level, but they are all very moun-
tainous and send down glaciers into the sea, and as they are
exposed to moist sea air on every side, the precipitation,
almost all of which takes the form of snow, even in summer,
is, of course, unusually large." [1]

Nordenskiold, having called attention to the absence of ice-
bergs in those parts of the North Polar area, where there is
no high land, goes on to say, " In the South Polar Sea ice-
bergs occur in great numbers and of enormous size. If we
may assume that they have an origin similar to those of
Greenland, it is probable that round the South Pole there is
an extensive continent indented by deep fiords." [2]

There is another kind of evidence forthcoming to prove
that the Antarctic continent cannot be wholly covered with
an ice sheet, but must have projecting mountain summits
above it. This is the occurrence of erratic blocks upon the
icebergs in the South seas, which must have fallen upon the
mother glacier from rocks projecting above it. I have
already described several instances of such blocks,[3] and will
add one or two more.

Lyell mentions how, in a voyage of discovery made in the
Antarctic regions in 1839, a dark coloured *angular* mass of
rock was seen embedded in an iceberg, drifting along in mid-
ocean in lat. 61° S. That part of the rock which was visible
was about twelve feet in height and from five to six in width,
but the dark colour of the surrounding ice indicated that
much more of the stone was concealed. Lyell gives a figure
of this iceberg with the stone on it, and says it was between
250 to 300 feet high, and was no less than 1400 miles from
any certainly known land. Captain James Ross, in his
Antarctic voyage in 1841-2-3, saw multitudes of icebergs
transporting stones and rocks of various sizes, with frozen
mud, in high southern latitudes. " His companion, Dr. J.
Hooker," says Lyell, " informs me that he came to the con-
clusion that most of the Southern icebergs have stones upon

[1] Island Life 131.   [2] Voyage of the *Vega*, i. 422.
        [3] Ante, pp. 145-6.

them, although they are usually concealed from view by the quantity of snow which falls upon them.[4] Other instances are mentioned by Darwin.[5]

Dr. Croll argues that the stratified character of the ice in Southern icebergs proves that the ice rests on a level bottom.[6] It seems to me to prove the very local origin of these glaciers, otherwise the viscous flow would have disturbed the stratification.

It is almost incredible with the cumulative evidence we thus have, that Croll should have so persistently maintained the opinion that the Antarctic continent and Greenland, instead of being mountainous elevated tracts, are low-lying areas. It is stranger to find him actually arguing that on the Greenland plateau, which cannot be reached without climbing high rocky cliffs, we must measure the depth of the ice by the height above the sea of its upper surface. He, in fact, argues that because Nordenskiold found the upper surface of the ice plain in Greenland to be 7000 feet above the sea, 280 miles from the coast, therefore it is 7000 feet thick, and says if the Antarctic ice sheet has an equal slope, this would give 35,000 feet as the thickness of the ice at the South Pole.[7] There is a similar inconsequence, it seems to me, in the measure he applies to the thickness of the Antarctic ice sheet. This he deduces from the height above the water which some icebergs reach. It is overlooked that the great bulk of if not all icebergs, have a much bigger diameter below than above, and the portion visible tapers off very materially, so that we have no scientific basis for postulating icebergs of 5628 feet in thickness, as Dr. Croll postulates, because Captain Smithers met with a berg which he calculated to be 580 feet high.

Dr. Croll seems to have had some suspicion that the case was very strong against him, and virtually admits that we can nowhere, even in polar latitudes, find masses of ice moving over level plains, still less ascending deep gradients at a distance from the declivities, which give impetus to forward movement. He explains this by saying that both in the Arctic and Antarctic regions the seas are so deep that the power

[4] Lyell's Principles, 10th ed. i. 381, 2.
[5] 1st ed. of Nat. Mag. Appendix, and by Hayes in the Boston Journal, iv. 426.
[6] Discoveries, etc., 235, note.   [7] Id. 245.

of flotation is called into action close to the existing coasts, and that the ice sheets have thus been broken up. But the soundings of Sir J. Ross close to the face of the Southern ice sheet show that the sea was sometimes not deeper than 570 feet, whereas Croll estimates the thickness of the ice sheet at 5000 feet. If so, why does it not move along the bottom of the Antarctic Ocean northwards, and ascend the slopes of the trough which appear to rise into shallower water to the North, which is less than 2000 feet deep for vast distances along the face of the ice cliffs, and is too shallow to float such an ice sheet as Croll postulates. At some places between three and four miles from the ice cliffs, water was found as shallow as 246 feet; and even at the distance of 120 miles the bottom was reached at 1140 feet.[8]

All this points unmistakably to the fact that the Antarctic continent, instead of consisting of a sheet or rather dome of ice, is in reality formed of rugged mountainous land, which nurses large masses of ice in the form of glaciers. It is not improbable that the great mass of this glacier ice is accumulated like a girdle at some distance from the pole, and that it is quite possible within this girdle there may be both open water at times and also vegetation, such as exists in the islands of the Arctic sea.

We are constrained, therefore, to confess that in whichever way we test the question of the movement of ice by virtue of its viscosity, whether on *a priori* grounds or by examining the largest masses of ice we can find, there does not seem to be any warrant for the view that it can move far over a flat surface, nor progress much beyond the foot of the high land, which, so far as we can see, always gives it its impulse.

If it cannot move along a flat surface, much less can it move a considerable distance up hill, as a considerable number of geologists have argued. No doubt so long as it retains its onward thrust it can move over slightly undulating ground, such as that described under the phrase *roches moutonnées*, but that it can move up long inclinations, sometimes several hundred feet in height, and traverse the troughs of deep seas, such as the necessities of some geologists postulate, seems to me completely contrary to the physical qualities of ice.

[8] Q. J. G. S. xxix., li. and lx.

" Glacier masses," says the Duke of Argyll, " are supposed to have moved freely up the slopes of their bed, to have ascended the land wherever it was encountered, and to have walked up the escarpments of mountains, more than 2000 feet high. We know that the descent of a glacier down the steep declivities of Mont Blanc is retarded by such an enormous amount of friction that the adherence of its substance is overcome, the base of it is, as it were, torn from its superincumbent mass, and the progress of the base is reduced to one-half of the rate at which the surface moves. We know that this is the result in a case where the force of gravity is at its maximum, and none of its momentum has been lost.[9]

This statement of the Duke seems to me to be beyond question. We have not exhausted the extravagant demands made by glacial geologists upon the undisclosed potentialities of nature. It is well known that in many localities the general drift of the boulders is in a different direction to that of the striæ, or the striæ themselves run in different directions, forming a kind of cross-hatching. To explain this paradox, a serious paradox to the advocates of ice sheets, it has been boldly argued by Ramsay and others, that ice in large masses does not all move in one direction, or in the direction where it meets with least resistance, and in accordance with the drainage lines of the country, but that within the ice mass itself there can be currents moving in different directions, at different levels, or even side by side, at the same time. I will quote one example of this form of argument.

In a paper on Drift, published in the *Geological Magazine* for 1874, Mr. Goodchild begins by stating that the striæ at low levels sometimes take a different direction to that at high levels. " Sometimes," he continues, "when the body of the ice was impelled up the valleys, indications may be found in the striæ at high levels to prove that the upper strata of ice were acted upon by currents flowing completely at right angles to the course of the upper parts." He then refers to the notion, started by Professor Ramsay, to explain this, not only that there were crossing currents in the ice sheet, but that it was quite possible that some of the higher

[9] Id. lix.

parts of it may have moved in directions directly opposite to those taken by those at lower levels. Mr. Goodchild explains in this way the transportal of drift in directions opposite to the movements of the ice as indicated by the scratches.

" We are therefore," he says, " led to conclude that the bottom in acting more under the influence of downward pressure than the higher parts moved downwards and outwards to the point where its outward motion would be stopped by the upflowing ice, while the higher strata, being more affected by the local set of the ice currents at the surface which were acting horizontally, and being less influenced by the overburden of ice, would move towards the head of the valley up to the point where the local ice would check or alter its course. In this case, therefore, it is easy to see how the two currents over any given spot may be moving in precisely opposite directions at the same time. An exact parallel may be found in the undertow flowing seawards from a beach, while a little higher up the breaker is advancing towards the shore."

". . . There must have been somewhere a point at the bottom of the ice at which the two opposing forces, the outflowing and the inflowing, must have been exactly equal in energy, and in which, therefore, the line of least resistance must have lain in an upward direction. This course, therefore, the ice would take ; and many, if not all, of the stones set in the bottom of the ice would go up with it. In proportion as the stones rose in the bottom of the ice, they would be brought more within the influence of the higher currents, and would in that way occasionally be carried backwards in the ice over the spot at which they first worked into it at a lower level. The crossing currents, before referred to, would tend to bring about a similar result. Many stones which were fixed in the crossing streams would, on the blending of the two currents, find their way into the body of the conjoined stream without first working downwards to the base. When once they were fairly into the ice, gravity would have but little effect upon their position, but they might be floated, so to speak, across wide valleys or over lower currents, transporting other boulders in different directions without working downwards to any appreciable extent." [1]

[1] Geol. Mag. xi. 504.

What a fantastic notion this is. Where is the experimental proof that such movements are possible even? To speak of crossing currents, as if ice were either vapour or liquid and followed the exceptional laws that sometimes rule matter in a state of very loose cohesion, when different strata are at different temperatures, is assuredly to equate incommensurable notions. Well may a correspondent who was never answered, and whose letter, signed Jack Frost, like a green boulder from Shap, closes the eleventh volume of the *Geological Magazine,* complain that Mr. Goodchild speaks of the separate and independent movement of the upper and lower strata of ice in an ice sheet as if it were an old established fact, adding, " Whilst ready to concede the fact that when the lower part of a stream of ice becomes impeded in its downward course, the upper part of the pent-up ice stream will rise over and flow on beyond the obstruction, I fail to understand the upper strata flowing in *the reverse direction."* So assuredly will every one close. The argument is, however, useful as showing the stress to which the uncompromising adherents of ice have been driven to maintain their case.

Let us now consider shortly another conclusion which it seems to me has been pressed with little consideration for the laws of physics. One of the anomalous and curious facts which has to be explained by any one who ventures upon a complete theory to explain the Drift phenomena is the transport of masses of rock from lower levels to higher ones. This has been observed in many places in Europe and in America.

The extent of this portage has been much overlooked, and I will quote some instances from the other side of the Atlantic. In the report on the Oriskany and Hildesberg erratic in Pennsylvania, we read how, on the sides of the Killahinny mountains, they have been carried up a slope of 7° two miles long to a height of 1300 feet, and in another place up a slope of 16° to a height of 1400 feet. One boulder of Hildesberg limestone perched on the summit of the Killahinny mountain had been lifted more than 1000 feet above its natural outcrop. Another boulder of the same limestone, 20 feet long, lies on the south side of the mountain, having been carried over its crest and several miles beyond. Sometimes, the report says, boulders have been carried from one mountain to another

across an intervening valley; sometimes lifted from a valley to the top of a mountain, and sometimes carried from one valley to another after being lifted across a mountain range. Hitchcock, Dana and other writers mention as a common fact the occurrence in North America generally of erratics at considerable heights, 2000 feet and more above the rock masses whence they were derived; and some of them 150 or 200 miles distant from their parent rock.[2]

At Areokartan, in Norway, Tornebohm found blocks at a height of 4500 feet which could not possibly have come from any places higher than 1800 feet.

Mr. Home found erratics of granite on Merrick, in Scotland, up to a height of 2764 feet, the highest point reached by granite in plain being only 2270 feet.[3]

This phenomenon is very difficult to explain by any ice sheet theory. If the ice sheet was sufficiently thick to act as a porter at the higher level, I cannot see how it can have received its loads at all, for, as we have seen, ice does not break up the bed over which it travels, and can only receive a load of rocks from peaks projecting above it. How, then, could it take up rocks from low levels and deposit them at high ones? The rocks *in situ* at low levels must have been smothered over with ice if the ice sheet was thick enough to deposit the erratics several hundred feet higher up. This is apart altogether from the difficulty I have already discussed in regard to ice moving up hill for long distances.

Dr. G. W. Wright argues that owing to the differential motion of the various superimposed layers of ice, in a moving mass of ice the boulders enclosed in it are subjected to a differential strain in which their upper portions are urged along faster than their lower ones; and he argues further that this must produce a movement slightly upwards, the result being that the boulders may in this way be gradually lifted up by the ice itself from lower to higher levels. He explains this by the fact that the boulder by intervening a barrier to the ice flow causes the ice immediately in front of it to stagnate somewhat, while the ice above moves on, and consequently the direction of least resistance must be slightly upwards. I confess that I do not understand this argument. In moving

[2] See Dana's Manual, 530, etc. [3] Quart. Journ. Geol. Soc. Glas., i. 135.

upwards at all the stone has to overcome the tremendous vertical pressure of the ice mass above it which assists any movement it may make downwards. Again, the ice is moving on in the layers below the stone as well as above it; why should the stone not therefore move diagonally downwards instead of upwards? Again, we know experimentally that the ice flows freely round all obstacles, and if there is any stagnation in front of a boulder, it must be limited to a very small layer indeed. Apart from this, if there is a tendency thus to move upwards, how is it there are any stones at all below a glacier? and how, again, can they possibly erode its bed? stones whose tendency to move is upwards cannot act as scrapers and chisels. In every way this explanation is faced, it seems to be fantastic and transcendental.

Let us now turn from the *modus operandi* of ice when moving in large masses to the products of its handiwork, especially the angular erratics and striated and rolled stones, etc., which it is credited with having distributed. It has been argued that these stones come from two different sources. That one section comes from the glacier's own bed which it disintegrates as it moves along, while another part comes from without it. I altogether dispute the former notion. The objections to it have been well stated by Mr. Irving.

" It has been suggested," he says, " that the freezing of water within the crevasses and pores of the rocky bed of the glacier, must by its expansion break up the rock surface, and thus furnish detritus for the glacier to carry away as the loosened materials are caught up by the ice. We must recollect that the water contained in this way within the rock is exposed to subterranean heat passing up by conduction from below, and that if this is slow owing to the low conductivity of the rock materials, the cooling effect of the ice of the glacier is *à fortiori* equally slow ; secondly, the actual surface of the rock at any given point is in contact with either (*a*) the water of the glacier stream which is not below 0° C., and therefore cannot freeze the water within the rock, or (*b*) in contact with the ice (or a stone stuck in the ice at the same temperature as the ice), in which case the ice may be either at or below 0° C., according to the pressure at the point of contact, as Helmholtz's reasoning shows. Ice at 0° C. has no power to

freeze water at 0° C., since with equality of temperature there can be no exchange of heat between; and if the ice be below 0° C., it can only be so at a pressure proportionately greater. This very pressure must be exerted upon the rock, and so counteract the expansive force of the water within the rock. The hypothesis seems to be wholly inadmissible. Further, the actual appearance of glaciated rocks shows that they have not been thus broken up by freezing water while the glacier covered them."[4] Mr. J. F. Campbell, a keen hunter of glacial phenomena, says he did not see how ice, creeping over a flat country, which it completely covered, could break up the rocks beneath in large masses instead of wearing them away by gradual and constant attrition.[5] In my view, the only product of erosion which comes actually from the glacier bed is the so-called glacier flour, i.e. the mud which is the result of the polishing and striating of its surface, and the rounding of the stones which are carried along by it.

Let us now turn to the other, and, as I hold, the only source of the blocks and boulders carried by glaciers. As a glacier moves down a valley, the exposed rocks on each side of it, broken by frost and rain, drop large masses and smaller fragments upon its back. These form a long line on each side of it, running underneath the exposed rocks whence they were derived, and are known to the Swiss as moraines. When two glaciers meet and join their current, the two moraines nearest to each other unite, and thence onwards the glacier carries three of them, one on each side called lateral moraines, one in the middle known as a medial moraine.

This and this only, so far as I know, is the source of all the unweathered and angular blocks borne along by glaciers. If glaciers had no crevasses or other fissures and holes in it, the only stones it would bear along would be the unweathered and angular erratics already mentioned. We know, however, that they also carry along a number of true boulders, which have been more or less abraded and rounded, and which bear signs of having been rubbed and worn. They are generally flat-sided and striated, and we can hardly doubt that they are stones which have fallen from the glacier's back down the crevasses and pits, have there been

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[4] Q. J. G. S. xxxix. 67.   [5] Geol. Mag. 1st series, ii. 188.

fastened in the ice foot like so many teeth in a gigantic file, and have acted both as eroding agents and been themselves eroded. Another set of stones, the typical rounded boulders, which occur so frequently in moraines, seem to be largely the product of sub-glacial attrition by water. As a glacier moves along, the friction upon its base, and also the heat of the ground, cause a certain portion of the ice-foot to melt, which is recruited by streams falling down crevasses, etc., the result of surfaces melting. This water forms underground streams beneath the glacier, and as it often flows down rapid inclines it rolls such stones as it meets with into boulders.

The rolled and striated stones just mentioned accumulate at the foot of the glacier in a rude unstratified manner, mixed with such mud as the streams have not washed away, and form a heterogeneous mass, which is pushed along by an advancing glacier and left behind by a retreating one to mark its furthest progress. It is known as a terminal moraine.

In the Alpine country, where glaciers and their work can be st died apart from extraneous elements, the only moraines known to us are the lateral and medial moraines already mentioned, and terminal moraines, all of which we can explain most clearly by the operation of existing causes.

Now it is a difficulty in the way of the Glacial theory, as ordinarily taught, that these moraines which we know to be the ordinary every-day products of moving ice, are quite different to many of the so-called glacial beds. They differ from them in being mounds and hillocks of heterogeneously mixed mud and boulders and sand, and not sorted into clay and sand and gravel, and spread out for miles with level or undulating surfaces.

Writing on this subject, Mr. W. M. Williams says very pointedly: " The most prominent and puzzling reflection or conclusion suggested by reading Mr. Geikie's description of the glacial deposits of Scotland, was that the great bulk of them are quite different from the deposits of existing glaciers. This reminded me of a previous puzzle and disappointment that I had met in Norway, where I had observed such abundance of striation, such universality of polished rocks and rounded mountains, and so many striking examples of perched blocks, and scarcely any decent vestiges of moraines.

This was especially the case in Arctic Norway. Coasting from Trondhjem to Hammerfest, winding round glaciated islands, in and out of fjords, banked with glaciated rock slopes, along more than a thousand ancient glacier valleys, scanning eagerly throughout from sea to summit, landing at several stations and climbing the most commanding hills, *I saw only one ancient moraine*—that at the Onfjord station, described in Chapter vi., of 'Through Norway with a knapsack.'"

But this negative anomaly is not all. The ancient glacial deposits are not only remarkable on account of the absence of the most characteristic of modern glacier marks, but in consisting mainly of something which is quite different from any of the deposits actually formed by any of the modern glaciers of Switzerland or any other country within the temperate zone.

I have seen nothing, either at the foot or the sides of any living Alpine or Scandinavian glacier, that even approximately represents the "till" or "boulder clay," and have met with no notice of such a thing by any writer on glaciers. Yet the so-called Glacial beds comprise vast deposits of this kind, covering many hundreds of square miles even of the limited area of the British Isles, and which constitute the main evidence upon which we base all our theories respecting the existence and the vast extent and influence of the "Great Ice age."[6] Mr. Geikie does not dispute this argument. He meets it by what lawyers call a confession and avoidance.

"We cannot fail," he says, "to remark that although scratched and polished stones occur not unfrequently in the frontal moraines of Alpine glaciers, yet at the same time these moraines do not at all resemble till or boulder clay. The moraine consists for the most part of a confused heap of rough angular stones and blocks and loose sand and *débris;* scratched stones are decidedly in a minority, and indeed a close search will often fail to show them. Clearly then, till is not of the nature of a terminal moraine."[7]

Again, he says, "We look in vain among the glaciers of the Alps for such a deposit (i.e. till). The scratched stones we may occasionally find, but where is the clay?"[8]

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[6] Quart. Journ. of Science, vii. 216.   [7] Great Ice Age, 60.
[8] Id. 61.

This fact is assuredly an awkward one for those advocates of extreme glacial views who follow inductive methods. An answer was forthcoming, however, from those who can always appeal to the transcendental when the real fails them, and accordingly another kind of moraine altogether was invented, which was supposed to be characteristic of ice sheets as contrasted with glaciers. This kind of moraine was first definitely described by C. Martins, who gave it the name of *moraine profonde*. The Germans call it *grund moraine*. A ground moraine was defined as a cushion of soft materials, intervening between an ice sheet and its bed, and not only made but also distributed by the ice sheet. I am bound to say that I utterly disbelieve, not only in the existence, but in the possibility of such a ground moraine under the postulated conditions.

How it is to be explained has been a puzzle to many others beside myself. If an ice sheet is an eroding instrument, pressing with enormous weight upon its bed, polishing and striating it, how can it, at the same time, deposit a layer of soft *débris* underneath its foot?

Such direct evidence as we can get certainly does not solve the paradox.

Sir Charles Lyell writes: "In 1867 I was able to pass for some distance under the terminal arch of the great glacier of the Viesch, a tributary of the Upper Rhone. It was in autumn (September 1st), and during the preceding summer the glacier had retreated many yards. Under the arch on one side was a floor of white granite, streaked, not only with straight furrows freshly made, but also with many parallel black lines, which had been ruled by fragments of soft, dark blue slate, fixed in the moving ice. According as the impinging stones had been harder or softer than the floor which they grated, they had cut rectilinear furrows in the rock which would last, or made superficial black markings which the glacier torrent of the ensuing winter would speedily wash away.[9]

"We have glaciers in Switzerland," says Dr. Southall, "has anyone ever known the till to accumulate under one of them? The Glacier de Bossons, about 1817, we believe, had

[9] Lyell's Principles, i. 376.

been continually advancing, until it not only covered part of a man's farm, but began to topple stones over the moraine down upon his house. The good Catholics then made a religious procession and stuck a cross on the moraine, when the glacier stopped; and it has now receded some four hundred yards. If this ice-sheet had possessed the power to lay down a stratum of " till " like those described by Mr. Geikie in Scotland, one might repair to Chamounix and dig up cats and dogs, and tobacco pipes, under a bed of true till. As it is, the ground is covered over with a desolate stone moraine at the lower border, and scattered blocks of stone above." [1]

Mr. Callard stated " that, in 1878, he had had the opportunity of studying the base of the Rhone glacier, which glacier has been slowly receding for a length of time. Beneath the glacier, as seen in the terminal ice cave, the ground was not ploughed, but only somewhat smoothed." [2]

Again Mr. Bonney says, " The glaciers of the Swiss and Savoy Alps have been retreating for several years; hence if anything like ground moraine existed, this (i.e. the summer of 1875) would be a very favourable time for observing it. In no case have I been able to find signs of any deposit resembling till or boulder clay; the detrital matter which is scattered, generally sparsely, over the slope left bare by the retreating glacier, has fallen from its surface, like ordinary terminal moraines. Further, by availing myself of crevasses, etc., I have made my way occasionally for some little distance beneath the ice. Nothing has been seen but bare rock, with now and then a film of mud or a passing stone. In short, the result of an experience of some years has convinced me that if anything like the till or ground moraine of recent glacialists exists in the Alps, it is a very local and exceptional phenomenon. It had sometimes occurred to me, as I believe it has to others, that much till-like matter might be produced from a residuum of stones and mud which had once been involved in the ice of the glacier. My more careful observations during the present year have shown me that these cases of included materials are more exceptional and less abundant than I had anticipated. [3]

[1] Southall, Recent Origin of Man, 487-8.
[2] Q. J. G. S. xxxix. 71.   [3] Geol. Mag. xiii. 197.

Thus observation completely confirms *a priori* reasoning on this subject, and the fact is allowed in regard to ordinary glaciers by such extreme glacialists as Dr. James Geikie. " It is needless," he says, " to refer one to the petty glaciers of the Alps and Norway, to prove that glacial ice cannot both erode its bed, and accumulate *débris* upon that bed at one and the same time. . . . No considerable deposit could possibly gather below Alpine glaciers like those of Switzerland and Norway."

So far so good, but it seems that, according to Glacialist reasoning, when the glacier leaves its inclined bed, and takes to travelling over a level surface, a very different process ensues, and we are told that underneath glaciers of the kind that invaded the low ground of Piedmont and Lombardy, *we know* that thick deposits of tough boulder clay crammed with scratched stones *did accumulate."*

This is certainly arguing at the point of the pistol. No attempt is made to show how it is possible, whether on an inclined surface or on a flat one, for an ice sheet to be at the same time an erosive instrument sufficiently erosive to be able to scratch and furrow rocks and to excavate lakes, etc., and to carry blocks of stones and shells for hundreds of feet up inclined slopes, as the same school of geologists demand, and at the same time to deposit a soft bed of materials, which must be quite different to a terminal moraine in structure, if it is to resemble the till and other glacial deposits. The problem is assuredly a gigantic puzzle, needing a definite solution. It is merely arguing in a circle, to say that because the drift is widespread in Lombardy and elsewhere, therefore it must have been the handiwork of ice sheets, and that therefore ice sheets must have possessed the special gift of so distributing it. I am as much at a loss to understand this transcendental reasoning.

If an ice sheet had sufficient energy in it to traverse a deep hollow like the Baltic, and carve out the so-called glacial grooves which have been said to occur in North Germany, I altogether fail to see how it could avoid sweeping away every trace of soft material with its portentous foot.

It is not pretended that when the glacier or the ice sheet reached the level ground its work as a porter of such materials ceased. Whether we test its supposed work in Eastern England among the different boulder clays or among the drift beds of

North Germany, we shall find everywhere a mixture of local *débris* of lias and of chalk in Eastern England, and of tertiary *débris* in Germany which came from the locality itself, and which is the best witness to the force at work having been a very active and erosive one. If it was an ice sheet, how came it then to have its foot padded with a ground moraine of such soft materials? and how, if it had, could it break up and grind down rocks *en route?*

It is as reasonable to argue that it could as to suppose we could scrape a man's bones without injuring his skin. It is assuredly contrary both to logic and all experience, that the same agent acting under the influence of the same forces, and with no ostensible reason, should, when cushioned with soft mud at one time, grind rocks to a smooth surface and cover them with striæ, and at another merely deposit the cushion on which it rests or pass over the soft beds of mud and clay without disturbing them.

Mr. Bonney says of Mr. Geikie's theory, " He speaks of the till as a *ground moraine* or *moraine profonde* formed between the glacier and the rock, while he attributes the majority of rock basins to the action of the glaciers. Now it appears to me, that if the glaciers could pass over considerable deposits of this *moraine profonde* without sweeping it clean away, then their action as erosive agents must have been comparatively feeble ; or if they could scoop out great rock basins, like the Alpine and (buried) Highland lakes, then they would have peeled off almost all the till from the land." " Difficult, as it undoubtedly is, to explain some of the lake basins, it appears to me that the great bulk of his evidence, with regard to till and other deposits over which ice streams have passed, shows how slight, under ordinary circumstances, is their erosive power ; and this has been confirmed by every journey I have made among the Alps. I may also add that, from study of the same regions, my faith in a *moraine profonde* is much shaken. I believe that except possibly as a very local and exceptional phenomenon, it exists solely in the imagination of the eminent geologists of whom Mr. Geikie is a disciple." [4] It appears to me, the analogy with a river, by which Mr. Geikie [5] seeks to escape from a portion of this

[4] P. 88.   [5] Nature, xiv. 85.

difficulty, does not hold. A flowing river, when its flow is gentle, does not erode at all. Even when it is moving rapidly it requires a considerable fall to do so. I have watched the very rapid rivers of Switzerland rushing over their stony beds without disturbing a pebble. When a river begins to deposit sand and gravel largely, its work as an erosive agent at that place is almost over, while ice moving over level plains is, *ex-hypothesi,* an agent sufficiently erosive to plough out great lakes, etc.

Matthew Williams says very truly, "That if glaciers are capable of eroding solid rocks, the slimy mud of their own deposits could not possibly have resisted them. The only case where this might have happened, is when a mountain wall has blocked the further downward progress of a glacier, or in pockets or steep hollows which a glacier might have bridged over and filled up; but such pockets are by no means the characteristic localities of till, though the till of Switzerland may possibly show examples of the first case. The great depth of the inland lakes of Norway, their bottoms being usually far below that of the present sea bottom, is in direct contradiction of this. They should, before all places, be filled with till, if the till were a ground moraine formed on land."[6]

Mr. M. Reade says, "It is possible to conceive that local conditions might exist at places favourable to the accumulation of matter under the glaciers, in the form of what is called ground moraine, but to assume, as is done by those who support the land origin of the till, that the very same agent, without any ostensible cause being alleged for the change, first ground the rocks to a smooth surface generally, and then very uniformly covered them up in the same track with a coating of till, in many cases over 100 feet thick, is, to my mind, a most unphilosophical begging of the question."[7]

Mr. T. B. Dakyns writes : . . . . "How uniform sheets of till should be left along a smooth valley or on open ground is not intelligible on any land ice theory. Geologists but conjure with the term *moraine profonde.* The ice that scaled and polished the solid rock could not at the same time be

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[6] Quart. Journ. of Science, vii. 236.
[7] Trans. Geol. Soc. Glasgow, vi. 273.

moving over a cushion of boulder clay. It must have been in contrast with the rock; it might accumulate the waste of the polished rocks as till in isolated sheltered spots; but not uniformly over the face of the country, and least of all where the underlying rock shows signs of glaciation; in fact, the mere instance of till itself negatives the idea." [8]

That a glacier, when it reaches level ground and is gradually stagnating, as we know it does, can and does move *for a short distance* over soft beds with little disturbance, is true enough. The fact has been observed in the Alps, and it has been also observed more recently by the American explorers of Alaska, but this is because it has then ceased to be an eroding agent. It is the case of the lower Rhine constructing its own aqueduct, but the lower Rhine is not an erosive agent. Where it is an erosive agent, namely in its upper reaches, it deposits no materials in its bed.

Let us proceed, however. The advocates of ice sheets not only argue that they could deposit materials on their own beds, but transport materials on a large scale beneath them and spread them out as we find them. Not that individual stones could be pushed along when more or less embedded in the ice foot, but that layers of mud, sand, and boulders mixed together could be thus transported. Not as a thin film which might possibly be frozen into the ice foot, but as continuous beds many feet thick, in some cases many hundreds of feet thick, upon which, so far as I can see, it would have no possible grip or hold.

Professor Young says : " Where I now write nearly 100 feet of till intervene between the surface and the carboniferous rocks, which, when exposed, show striations having the usual compass-bearing for this neighbourhood." Most of the till is derived from the carboniferous rocks in the neighbourhood, but numerous other boulders in it show it also brought materials from a wider area. How is this to be explained ? When," says Mr. Young, " we are asked to believe that the till is not merely a product of land ice, but accumulated on land under a moving ice sheet, and are further asked to accept the erosion, the contortion and the non-disturbance of stratified beds as alike the accompaniments of the passage,

[8] Geol. Mag. 1875, 169, 170.

not of land ice, but of the morainic matter pushed forward by it, we are practically asked to give up the attempt to solve a difficulty which has never been really faced. Again," he says, " since from 50 to 60 per cent. of till consists of impalpable mud, how was it it was not washed away by the waters constantly present beneath moving ice ? " and he concludes, " My wish is to learn how the 100 feet of till were accumulated, for even if they were piled up under a thinning glacier, it still remains to be explained how so vast a quantity of detritus could have been worn from a small area by an agent steadily diminishing in force."[9]

Mr. Searles Wood, junr., says " The cliff on the Suffolk coast, from Kessingland to Yarmouth, is formed by a continuous deposit of undisturbed, horizontally bedded sand, containing marine mollusca, etc., and covered by morainic clay, often 20 feet, and averaging fully 12 feet in thickness, the junction between the two being undisturbed except at one or two places. Now, I say," he continues, "it is a physical impossibility that the whole of this thick sheet of morainic clay, 15 miles wide by 40 long, can have been dragged for 40 miles over the sand without either crumpling or destroying it in the least, and without incorporating part of such sand and of the contorted drift bosses into itself. The impossibility becomes more striking if we supplement the weight of this sheet of clay by the manifold greater weight of the ice which Mr. Geikie contends overlaid and dragged it."[10] Elsewhere he points to the thread of sand packed with the remains of marine mollusca with valves adherent, discovered by Professor Hughes and Sir C. Lyell *in the midst* of the chalky basement clay of Holderness, which is similar in character and structure to the chalky clay of Suffolk, as a very conclusive case.[1]

The occurrence of shells intact among the glacial beds, is another critical difficulty for the advocates of ice sheets. Thus, to quote an instance or two only : When Sir Charles Lyell visited Holderness in 1869 with Mr. Hughes and his nephew, they found in the midst of unstratified chalky clay of the lower part of Dunlington cliff a thin streak of greenish sand embedded in the clay, which, according to the

[9] Geol. Mag. 1878, 162–164.   [10] Geol. Mag. 1878, 188.
[1] Id. 335.

description of Mr. L. Lyell, was crammed with perfect speci-
mens of Nucula Cobboldiæ, some of these, Mr. Lyell adds,
had, he believes, the two valves adherent, and one, *astarte*,
certainly had.

Professor Phillips, speaking of the beds at Ridgmont, in
Yorkshire, says, " Besides the strong shells of turbo littoreus,
purpura lapillus, and buccinum undatum, we have mya
arenaria, tellina solidula, t. tenuis, mactra subtruncata, car-
dium edule, etc., and it is certainly very strange to discover
these and other tender shells in as good state of conservation
among the twisted and confused lamina of so coarse and ir-
regular a deposit as that in the vicinity of Ridgmont." [2] It
would indeed be strange if the disturbing element were an
ice sheet, crushing and scraping over these delicate shells.

This is not all, however. We are gravely told, not only that
this agent, which is supposed capable of eroding deep lakes
out of solid rock, could gently sweep up the bottom of the
North Sea and the Irish Channel and transport it and its
containing shells for many miles, and push them up slopes of
several hundred yards in length without, in many cases, break-
ing or injuring the shells.

This notion was, I believe, first pressed by Dr. Croll, and
the view is concisely stated by one of its supporters, Mr.
Tiddeman, who says : " It is a point insisted on by some
geologists, that wherever rolled stones or marine shells are
found in the boulder-clay, it must be of marine origin. I do
not think either of these characters is infallible. Mr. Croll
has shown that the Caithness Till, which contains shells, need
not necessarily be marine, but may have been farmed by the
ice sheet working over a previous sea-bed, and pushing the
shells on to the land. In this way shells scratched and broken
may be found at very much higher levels than the sea in which
they lived and died. They are as much boulders as the
scratched stones alongside of them, and are no more evidence
of the drift in which they lie having been formed under the
sea than Spirifers and Producti found in limestone river gravel
would be proof of its being marine. In very many places the
ice sheet must have passed over what had previously been the
sea-bed, and if its course took it thence inland, we should be

[2] Op. cit. 303.

surprised not to find sea-shells mixed with the drift formed by it." [3]

Mr. Belt has adopted the same view. Thus he says of the shells at Moel Tryfaen : "They are just where they ought to be found, on the supposition that an immense body of ice coming down from Northern Ireland, from Scotland, and from Cumberland and Westmoreland, filled the basin of the Irish Sea, scooped out the sand with the shells that had lived and died there, and thrust them far up amongst the Welsh hills that opposed its course southward and around the great height of which Liverpool forms the apex." [4]

In regard to this theory, we can hardly approach it seriously. Where, in glaciated regions like Greenland, do we find the ice being thrust up into the country from the sea in this fashion to the extent of 1400 feet ? How could molluscs live at all in a sea occupied by such an ice sheet which was scouring the bottom ? How could very delicate shells be found entire and unbroken, as Mr. Etheridge affirms they were, at Moel Tryfaen,[5] after such a journey and under the foot of such a crushing mass ? How could the shells from various depths and habitats be brought together at one spot in this fashion ? How could they live along shores lined with immense masses of ground-ice engaged in making boulders and boulder-clays ? It must be remembered that Forbes has shown that most of the shells found in the high-level drifts are littoral shells ; that is to say, the molluscs must have lived where the ice would be the most developed.

I might go on with my comments, but I prefer to conclude with some observations of Professor Bonney, who has put this view, as it seems to me, *hors de combat* in some characteristically incisive phrases : " Apart," he says, " from the difficulties of a glacier thus walking so far up-hill, and of shells having escaped utter smashing in this uncomfortable mode of transport, Mr. Belt has forgotten that Wales was a centre from which radiated glaciers, and at one time an ice sheet, which surely would have warded off from its own hills the northern intruder." [6] Again, he says, " Mr. Belt appears to

[3] Journ. Geol. Soc. vol. xxviii. p. 471.
[4] Nature, vol. x. p. 26.   [5] Geol. Journ. vol. xxxvi. p. 355.
[6] Nature, x.

forget that shells have been found not only at Moel Tryfaen, but also near Llyn Ffynnon-y-gwas, about two miles west of the peaks of Snowdon. Does Mr. Belt mean to say that Snowdon could not protect itself in the heart of its own domain better than this? If the lake mountains had an ice sheet, surely Snowdonia." "I think it in the highest degree improbable that the Vale Royal shells could be brought to their present position (more than 1100 feet above the sea) by any ice sheet without the cold being enough to cover *all* the higher ground in Britain with ice, and so protect it." Again, "My contention was, that the enormous force which would be exerted on beds scooped out as described, and shoved some 1500 feet up-hill for miles over broken ground, would crush the shells to a far more comminuted state than they are now in."[7] Mr. A. H. Green adds to these arguments that the shell-bearing drift gravels are stratified: "I can speak," he says, "to those in the neighbourhood of Macclesfield, which run up to 1100 feet above the sea, being also very delicately current-laminated. I am puzzled to imagine how this structure could be obtained, if the gravels were brought to their present position in the way Mr. Belt supposes; indeed, its presence seems to me fatal to his hypothesis."[8]

Lastly, as Mr. Mackintosh says, "An intimate acquaintance with the character of the Moel Tryfaen deposits precludes the idea that all the shells, together with the erratic stones, were pushed out of the beds of the Irish Sea as far south as Moel Tryfaen; for, if it were a true explanation, the shells and erratic stones would have diminished in number the higher up they were pushed. But, on the contrary, the shells and erratics in the drifts near the sea are fewer in number than on Moel Tryfaen. This theory would likewise require to invest the land-ice with the power of rounding the pebbles derived from the upper part of the hill, and laminating the sand and fine gravel; for it ought to be remembered that though the sand and gravel are, in places, much contorted on Moel Tryfaen, the contortion was evidently in many instances produced after their accumulation."[9] The same experienced geologist again says: "The idea of shell fragments having been pushed up-hill along with portions of existing sea-beds

[7] Id. p. 85.　　　[8] Id. p. 105.　　　[9] Journ. Geol. Soc. vol. xxxvii. p. 351.

is opposed by so many facts as to render it altogether untenable." [1]

Let us now pass on. How can we account for sporadic pockets and layers of sand and gravel often stratified or intercalated in the boulder-clay, if the latter was the product of a uniform force scooping in a uniform direction under uniform conditions like the supposed ice-sheet?

Mr. Goodchild quotes an interesting case of this kind, which is very important for our argument. He says, "In one of the Settle and Carlisle Railway cuttings, near Horton-in-Ribblesdale, the line has passed through a deposit of tough clay, full of glaciated boulders, and quite of the character of ordinary till. In one part a flask-shaped deposit of finely laminated and false-bedded sand and thin seams of clean gravel occurs, surrounded, certainly on three sides, by the clay drift. There is not the slightest sign of any contortion; and the deposit is clearly not due to river action, because the sides of the nest close in rapidly above, and the railway cutting shows plainly enough that the sand does not extend even as far as the western bank.

If this stiff clay, full of glaciated stones, is part of a *moraine profonde,* how is this undisturbed nest of sand, which extends upwards for at least 30 feet, to be accounted for? The ice that this drift is supposed to have accumulated under, cannot have been less than several hundred, perhaps it was more than a thousand feet, in thickness over that particular spot; it is, therefore, obvious that the forward movement of such a mass a single inch must have resulted in the kneading up of all the sand into the drift in such a way that every trace of stratification would be obliterated." [2]

Again, in speaking of the frequent occurrence of entirely undisturbed beds of finely stratified and incoherent sands and sheets of laminated clay, intercalated with the till, the same author says : " When we try to explain the presence of seven or eight or, in some cases, as many as eleven beds of till, interstratified with undisturbed beds of sand and finely laminated clays by the *moraine profonde* theory, we meet with complete failure. It does very well where we have only to account for the origin of an unstratified mass of stiff clay full of glaciated stone

[1] Id. vol. xxxviii. p. 195. Journ. Geol. Soc. xxxi. 77, 78.

whose longer areas are nearly parallel to the underlying rock surface. . . . But these soft beds of sand and clay occurring interstratified with the till, could not have escaped violent contortion. They would be much more likely to be kneaded up into the clays until every trace of their existence was lost, if the ice ever advanced a single inch over them. . . . If it was a *moraine profonde* that took the Galloway boulders right across England to the North Sea and tore up buckram from the bottom of the valley of the Eden and afterwards transported it up the slopes of Stainmoor . . . how can we account for the presence of entirely undisturbed beds of finely stratified and incoherent sands, and sheets of laminated clays in and beneath the till ? " [3]

How again, by any theory of ice sheets, are we to account for the long mounds known as kames, eskers or asär, to which we shall revert in a subsequent chapter ? These mounds, often stratified, containing marine shells, and situated as they are in the very focus of where the ice sheet would be most energetic in its operations, seem absolutely incompatible with such a cause.

How again, on this theory, can we explain the very local distribution of till properly so called ? So local that Agassiz had never seen it till he came to these islands ? How again, for its distribution chiefly in maritime districts, as in the country where I am writing, where, as Mr. Aitken has shown, the great deposits of till are on the western side of the Pennine chain, and that when we cross it becomes quite insignificant comparatively ? How, also, are we to account for its being frequently of different colour and texture, according to the nature of the subjacent rock, and at others uniform, independent of the subjacent strata ? for an ice sheet is, *ex-hypothesi*, a huge mass moving for untold centuries with one uniform set of conditions, and therefore likely to distribute the products of denudation uniformly.

The mode in which the materials of the drift are distributed is also strongly against its having been deposited by an ice sheet. An ice sheet or a glacier does not deposit the stones which it carries according to their size. Large and small blocks are carried at the same rate, and deposited indiscrimi-

[3] Goodchild, J. R. G. S. xxxi. 93.

nately. Now it has been observed on the continent of Europe, that the boulders are roughly sorted according to their size. Sefstrom long ago remarked that the Scandinavian boulders diminish in size as we go south in Germany. The same phenomenon was observed by Murchison and his companions in Russia. Thus, while near Moscow the granite and diorite boulders rarely exceed 0·60 to one metre in size, about St. Petersburg they are often several metres in diameter. The same general law has been noticed by Dana to prevail in America. Thus he says, the material, though varying much in different regions, is in general coarsest to the north, and becomes gravel and sand, without stones, or only small ones, towards the southern limit of the drift region. Nearing this limit, it stretches further south in the north-and-south valleys than on the hills.[4]

This sorting of the drift according to its size and specific gravity is quite contrary to the action of ice, whose deposits are not sifted in this way.

Again as we go south, both in Europe and America, the drift with boulders gradually and continuously gives place to drift without boulders, and to great stretches of more or less clear sand or clay, and these again to wide stretches of brick earths and loams, variously named Chernozem, loess, etc.

The same is true in England. Here, as elsewhere, the boulder clays, and the associated sands, of the mountain districts of the North, give place to the great sheets of gravel of the Midland countries, and these, again, to the brick earths and loams of the Thames valley and the Southern counties. The continuity between these deposits is complete, and it seems to me to show that here, as elsewhere, the drift deposits are rudely distributed according to their specific gravity as we march southward. The condition of things is quite inconsistent with the operation of an ice sheet. It has been argued that the glacial beds are suddenly cut off about the latitude of the Thames. To this I altogether demur, the fact being that the number and size of the erratics diminishes rapidly as we approach the south of England, but they occur even in the very south.

Again, it has been noticed that in Germany the Scandinavian

[4] Manual, etc. 529.

boulders, composed largely of primitive rocks, do not extend as far south as the secondary and other boulders which have not come so far, so that the fan-like drift-beds of Germany, which are distinguished by such an abundance of Scandinavian erratics, are bounded all round their outer edge by a zone marked only by less travelled erratics.

Again, the blocks themselves, especially in Germany, instead of being sporadically distributed, are arranged in groups and alignments separated by barren areas.

Again, in certain places they are chiefly found on the surface, and in others are nearly always buried in sand and clay. They chiefly occur too on hillocks and on high ground, and are more infrequent on the level country. In one or two cases, such as the district of Orel in Russia, and Wisconsin in America, the drift seems to be entirely absent, as if they had escaped being overrun by the force, whatever it was, that distributed the drift elsewhere. All these facts seem to me to be absolutely inconsistent with the operations of great ice sheets.

Again, how are we to account by this theory for the mixture of rocks which have come from different directions? Thus in Saxony we find Scandinavian erratics from the north mixed with erratics from the south.

The same is the case in various parts of Central and Eastern England.

This mixture of *débris* coming from different directions is assuredly inconsistent with a great ice sheet having been the distributor of the drift.

If we examine the mountain districts again, we shall be struck by the fact of the occurrence in many places among the hills of large masses of rolled gravels. The heads of the valleys are often choked with beds of sand, gravel and clay, and masses of these soft *débris* occur right up to the heads of mountain passes, as may be seen in travelling along the Highland Railway, etc. Assuredly they would have been swept away completely if a huge ice sheet had ever moved over the face of the country.

Again, the occurrence of the enormous boulders of chalk, etc., in the drift seems to be very strong evidence against the movement of ice sheets where they are found. It is a mistake

to suppose they have only been discovered in the Cromer cliffs.

In the 9th volume of the *Geological Magazine*, pp. 403-408, Mr. Bonney discusses the occurrence of a very large mass of chalk in boulder clay in the Roslyn Hill clay at Ely, and which he treats as a boulder similar to the masses of chalk in the Norfolk cliffs. He refers to a similar mass, described by Professor Morris in the 9th volume of the Journal of the Geological Society, 430 feet long and 30 feet thick of oolite buoyed up in the drift.[5] Judd has discovered larger masses of erratic marlstone, and according to Sir A. Geikie, the lias of Lucksfield is an erratic. See also *Geological Magazine*, vol. v. p. 409, where Mr. Fisher mentions other similar masses.

As we have seen, glaciers do not break up their beds when moving, and if they did, how could they drag up from their beds these immense masses, in some cases several hundreds of feet long, and move them about underneath them, and cover them and underpin them with the sand and clay, which lie bedded round them, roughly stratified ? Assuredly, as Professor Judd urged, such a process is impossible.[6]

Lastly, it seems to me very difficult indeed to account for what is generally looked upon as the chief evidence of ice sheets, namely the continuous striæ running over the crests of the hills, and over high saddles and cols, and disregarding the general contour of the country by means of this agency. Ice alone will not make grooves and furrows in rocks. In order to do so it must be shod with stones to act as graving tools. But if the tops of the mountains were buried by an ice sheet, where could the stones be derived from ? As we have seen, the stones carried along by a glacier are not derived from the break-up of its bed, but from the *débris* of exposed rocks which rise above its surface. We can, therefore, as little account for these striæ on the plateaux as we can for the masses of clay occurring in similar places by the operation of ice sheets.

All these facts, and they seem to me to be cumulative, are absolutely inconsistent with the operations of great ice sheets moving independently of the contour of the country, and they seem to me to establish the conclusion that such ice sheets are neither consistent with the laws nor with the facts of nature.

[5] See also Journ. Geol. Soc. xxvii. p. 252.   [6] See Q. J. G. S. xxxvi. 528.

CHAPTER XVI.

THE ACTION OF LAND ICE IN THE SO-CALLED GLACIAL PERIOD, AS IN OUR OWN TIME, WAS LIMITED TO ICE IN THE FORM OF GLACIERS.

"The present knowledge of the whole subject" (i.e. the Glacial period) "is in an extremely unsatisfactory condition; and in this, as in other things, the first requisite of progress is to squarely face the extent of our ignorance upon the question."—(WRIGHT, Ice Age in America, 445.)

Geographical limits of the drift phenomena—Contradictions involved in the theory of ice-sheets—The Lofoden Islands with their sharp contours interposed in the midst of the postulated North Sea ice-sheet—M. Williams—Pettersen's observations on North Norway prove the Scandinavian ice did not advance much beyond the coast —Similar observations of Stanley—The Faroes had their own local glaciers, Geikie and Helland—The evidence of the Shetlands and Orkneys, as to a North Sea ice-sheet—Milne Home, etc.—Evidence of the North-East Scotland—Jamieson—Evidence of the mollusca opposed to an ice-sheet having occupied the North Sea—The evidence of the insular fauna and flora are the same—Reasons against a Scandinavian ice-sheet having overwhelmed the Baltic and North Germany—Norwegian blocks not found in Germany— The blocks found there have travelled in different directions—This is inconsistent with an ice-sheet—The striæ agree with the boulders —The striæ in Scandinavia, for the most part, follow the valleys —Evidence of the striæ on the borders of the Baltic and the island of Gothland—Evidence of the shells and stratified drift of Germany—The Alpine district—The crests of the Alps unweathered, showing they were not covered with ice—Evidence of several large glaciers, and not of one ice-sheet in the Alps in pleistocene times— Von der Linth and Guyot—Judd—The Jura—Falsan—The Vosges, etc.—The Pyrenees—Great Britain—Criticism of the theory of a Scotch ice-sheet—Dugald Bell—Jamieson—Somervail—Bryce— The Duke of Argyll—Belt—Criticism of ice-sheet in England— Cumberland etc.—Geikie—Goodchild—Harkness—Mackintosh— Ward—Dakyns—Lancashire, etc.—T. M. Reade—Aitken—Ricketts —Wales — Keeping — H. B. Woodward — David — Central and Eastern England—Crosskey—Woodward—De Rance—Jukes Brown —Searles Wood—Seeley—The alleged ice-sheet in the Irish Sea— Evidence of the mollusca, pebbles, etc.—The Isle of Man—Ward— Anglesea—Ricketts—Ireland—Evidence of local glaciers there— Hull—America—No evidence of the supposed ice-sheet west of the

Rocky Mountains—Whitney—Clarence King—J. C. Russell—J. Dawson-Gilbert—Occurrence of marine shells and remains of land plants in the clays opposed to an ice-sheet—Divergent direction of the drift in America—Other general objections to an ice-sheet—Evidence of local centres in America—Newfoundland—Prince Edward's Island—Dawson—Murray—Canada—Logan—Murray—Chalmers—Dawson—United States—Winchell—Rogers—Wright.

In the previous chapter I have tried to analyze the conditions under which land-ice moves, and which must have limited its capacity for distributing the so-called drift, and to show that it is impossible to attribute to it the work which the champions of ice-sheets have so commonly postulated in recent years. We cannot reconcile their postulates with the physics of ice and with its known properties. We cannot concede to it the power of distributing the drift as we see it distributed, and of explaining the complicated phenomena of the so-called ice age without traversing the elementary laws of hydrodynamics, and making impossible demands upon the mechanical forces which ice in any shape is endowed with. It remains to analyze the observed facts of geology, and to see what they really teach us when divested of *à priori* hypotheses, and how far they stultify our inquiry by making demands upon physics which are incompatible with the laws of Nature. In other words, to see how far these geological facts, when analyzed, support the vast induction which the extreme glacial school has built up.

We will begin with Europe. In Europe the champions of ice-sheets have argued that one vast blanket of ice, culminating in Scandinavia, once spread fan-like over the North Sea, marched westwards and overwhelmed the Faroes, the Orkneys and the Shetlands, Scotland and the North of England in its folds, and advanced as far as the "hundred-fathom line" in the Atlantic, where it is supposed to have terminated in a vertical cliff of ice like the Antarctic ice is said to do. While this was its western limit the same ice-sheet is supposed to have traversed the German Ocean and the Baltic, and to have travelled over the plains of Russia and of Germany, as far as the line more than once traced in the foregoing pages, which marks the further limit of the southern and eastern distribution of the northern drift. Such is the vast extent which a large number of students of glacial phenomena are

willing to concede to this great ice-sheet, and which they deem not only reasonable, but to be actually necessitated by the evidence.

We have already seen that such an ice-sheet, in order to move at all, must have been of portentous thickness at its culminating point in Scandinavia, that, if so, it must have entirely covered the Dovrefelds, and that this covering would cut off the supply of the very boulders whose occurrence on the east coast of England and in Central Germany is one of the main proofs relied upon for its existence. We have also shown that, as a matter of fact, the higher parts of the Dovrefelds still retain their rugged unweathered aspect, and have not been ice-worn, showing they were never so covered, so that the theory we are criticizing begins with a contradiction.

Again, the modern glacial doctrine separates the phenomena into two phases, one of them supposed to mark the greater or primary glaciation, when the ice-sheet is supposed to have moved over the country independently of its contour, and the other when it had shrunk into the hills, and when the ice moved in the form of glaciers only. Now, in regard to the phenomena of the drift as presented in Germany, Russia, etc., if it is to be explained by land-ice at all, it is clear that it must be explained by an ice-sheet, and not by glaciers, and it must have been the result of the major glaciation. If so, the striæ and grooves on the upper levels in Scandinavia, which in places seem to be entirely irrespective of the contour of the ground, ought to be in the same direction as the movement of the boulders. This is by no means the case, as may be seen by comparing the lines of migration of the boulders with the direction of the striæ as marked on the maps which have been published by Kjerulf and others. The stones again show a more or less fan-like distribution, pointing to their having spread out as they travelled, while the striæ on the higher levels are for the most part parallel. These facts involve irreconcilable contradictions, if we are to attribute both the striations and the boulder drift to the movement of an ice-sheet culminating in Scandinavia.

Let us now criticize some other factors in the theory as generally taught, and, first of all, the supposed extension of a

Scandinavian ice-sheet across the North Sea to the British Isles.

It is quite clear that an ice-sheet enveloping the greater part of Northern Europe, and occupying, *inter alia*, the basin of the North Sea for any considerable time, must have polished and rounded the surface of the rocks it mantled. This is quite inevitable. If, therefore, we find that there are now standing in the very midst of where such an ice-sheet must have passed on its way from Scandinavia to Britain a large number of sharp-edged rocks, showing no trace of any such abrasion, we may be sure that it cannot have been present there. We can fortunately appeal to such an *experimentum crucis* in the case of the Lofoden islands, which lie off the coast of Norway and extend over several degrees of latitude.

In regard to these islands, Mr. M. Williams says, "I found in the first place that all along the coast from Stavanger to the Varanger fiord, every rock *near the shore* is glaciated. Among the thousands of low-lying ridges that pier above the water to various heights, none near the mainland are angular. . . . . The rocks which constitute the extreme outlying limits of the Lofoden group, and which are between sixty and seventy miles from the shore, mineralogically correspond with those near the shore, as the sketch of three characteristic specimens plainly shows. Mr. Everest very aptly compares them to sharks' teeth. Proceeding northwards these rocks gradually progress in magnitude, until they become mountains of 3000 to 4000 feet in height ; their outspread bases form large islands, and the west fiord gradually narrows. The remarkably angular and jagged character of these rocks when weathered in the air renders it very easy to trace the limits of glaciation on viewing them at a distance. The outermost and smallest rocks show from a distance no signs of glaciation." [1] This fact is assuredly worth a peck of supposition. How, if the Scandinavian ice-sheet was big enough to cross the Baltic, was it possible for these jagged rocks to have escaped ? They must have succumbed to its embrace.

Leaving the Lofodens and turning to the coast of Norway it-

[1] Quart. Journ. of Science, vii. 232, 233.

self, we have similar evidence in some remarkable papers by Karl Pettersen, a most experienced observer, published in *Nature*, vols. 32 and 34. Evidence is there collected to show that the ice that gathered on the northern Norwegian highlands, far from having traversed the North Sea, did not even reach the small islands off the Scandinavian coast. He says with justice that if the ice streams from the west of Norway have, as assumed, moved forward, and filled not only the fiords to the bottom but the entire North Sea to England, the Channel from the bottom of the Bals fiord to the girdle of rocks and holms off the coast known as the Skjaergaard, should also have been filled with ice. "If this had been so, marked traces of such an ice stream would, no doubt, have been visible from the very bottom of the Bals fiord right out to the Skjaergaard, but the researches made here point in a different direction." [2]

"About eleven kilometres from the mouth of the Kval Sound, in the open ocean, lies the little Ris island, surrounded on the south-west and north by a great number of tiny islands reaching a height of 100 metres." These islands are composed of a granitic gneiss, well adapted to retain traces of polishing and smoothing, yet none such exist, nor any striæ, except at a very low level, lying within the littoral belt, where the sea is continually washing, and Pettersen says he had come to the conclusion "that the smoothing phenomena observed there must be ascribed to the erosion of the sea."

Again, he says, that on the Ris island and surrounding islands not a single boulder or travelled block, whose place of birth could have been on the Upland behind, has been found. "Nor can this be explained by the fact that the glimmer slate, interspersed with crystalline chalk, quartz, and sandstone-like slate of the Bals fiord and towards the Troms island, might have been disintegrated, for many blocks of slate which are supposed to have been derived from Scandinavia, occur scattered over the Central European high plateau, and if these were able to resist the destructive agencies during such a long transport, they would decidedly have done so here, where the distance is so short." Among the minerals found in the Bals fiord, however, are some such as gneiss, amphi-

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[2] *Nature*, vol. xxxii. p. 202.

bolite, eclogite and saussurite, whose composition would enable them to resist the severest destructive agencies, even during the longest journey. The total absence of foreign travelled blocks on Ris island and the adjacent holms is, therefore, hardly to be reconciled with the supposition that continuous streams of ice from the mainland moved thither. Neither is moraine drift met with about the archipelago in question.

In regard to the Troms island, most of the stones embedded in the boulder drift found on it belong to the island itself, and none can with certainty be derived from the mountains beyond the island. These drifts must, therefore, be assigned to local glaciers, which at one time partially covered it. The striæ are only found on the littoral belt, while foreign travelled stones, and those of different kinds of granite, only occur at low levels, up to a height of thirty-eight metres. The elevation at which they occur indicates that the transport of these foreign blocks can only have begun at a period when the more continuous layers of inland ice were broken up.

The bottom of the Bals fiord consists of a stratum of saussurite gabbro, a remarkably tough rock. In spite of the most careful search on the Troms island, which is most advantageously situated for receiving any fragments carried by the ice stream in the fiord, " the scientist has not succeeded, either between the loose, solitary blocks or in the glacial drift itself, in discovering a single fragment of saussurite gabbro. There is therefore every reason to assume that the ice streams which moved down the Bals fiord cannot have reached the Troms island. . . . From impressions during my frequent journeys along these inlets (in the channels north of Tromso, such as the Kval Sound) I can with confidence assert," says our author, " that no travelled blocks will be found here either, whose birth-place was on the Upland behind. A few years ago I examined the greater part of the mouth of the Bals fiord. I have not observed there either, travelled blocks transported from the head of the fiord. I have therefore come to the conclusion that the continuous ice stream of the glacial period cannot have reached beyond the basin of the fiord. From the premises thus set forth we may draw the deduction that the

inland ice in North Norway, during the glacial age, did not move forward through the sounds to the ocean, while we may presume that neither did they travel through the Balsfiord."

Pettersen goes on to say, " that the district to which these direct observations apply is a very considerable one. It includes fiords and sounds close upon 120 kilometres in length, and a coast line ninety kilometres long, and, what is most important, the channel in question is one which, by its orographical structure, would offer one of the most suitable routes for the flow of the ice. If the inland ice has not been powerful enough to move forward by this channel, there is, I believe, little probability of finding a single channel in Northern Norway, in the district between Salten and the North Cape, by which the inland ice has moved to the coast."

These being Pettersen's conclusions as to North Norway, he goes on to argue that the evidence on the spot points, as à priori it should point, to the glaciation there having been quite as, if not more, severe than that of South Norway. He quotes as a proof of this the ice masses in the Sogne fiord, which were probably from 1700 to 1800 metres in depth, while a granite boulder has been found on the peak of the Great Jerta, a mass of amphibolite slate, rising to a height of 1569 metres. This being so, it follows, argues Pettersen, that inasmuch as the inland ice in Northern Norway was not powerful enough to move to the ocean along the sounds, there is but little probability of the conditions having differed in this respect in the southern part. . . . " My deduction, therefore, is that the theory of the ice streams from the Scandinavian peninsula having advanced and covered the North Sea, the Baltic, and reached the Central European plain, England, the Orkneys, and the Shetlands, cannot, with the facts at our disposal, be accepted as a scientific doctrine."

In another paper he says : " From what I have advanced here as regards the blocks during their journey through the Bals fiord, it seems clear that their transport cannot be ascribed to a moving stream of inland ice. The sharp line of demarcation above which no blocks are found, seems in itself to demonstrate this." He attributes the distribution of these boulders to drift ice or shore ice, and this accounts for the mixture of coast granites with those from the Divi valley

along the broad sounds about the mouth of the fiord where the in and out flowing currents had alternate sway.[3]

Speaking of the fact that in the outer sounds on the Norwegian coast the erratic boulders are only found at low levels, namely from the shore line to 120 feet, he goes on to say, " It is particularly significant that the blocks are not found above a height of 120 feet along the low transverse ridge which runs from the Bals fiord on one side, westwards, to the Malanger fiord on the other, eastwards, to the Lyngen and Ulf's fiords, and so with the Malang isthmus. From this, also, it is clear that the inland ice cannot have moved forward across the Slet mountains and the isthmus between the Bals and Malanger fiords." [4]

Pettersen goes further, and adduces very strong evidence from the character of the blocks on Troms island for the conclusion that they have come from the Kval island to the west of it, and that their transport, instead of having been from the mainland seawards, has been in the opposite direction, namely, from *the coast landwards.* Of this latter conclusion, he says, he is fully convinced, and he attributes the transport of the blocks to icebergs.[5]

As the matter is one of considerable interest and importance, considering the influential reputations involved, I will quote yet another opinion. In a paper read before the Geological Society in 1887, Mr. W. F. Stanley, F.G.S., speaking of the result of his observations in Norway, says, " The aspect of the coast for hundreds of miles consecutively has a uniform character of jagged and pointed rocks nearly to the sea level. At the mouths of the fiords the rocks are more rounded, particularly at heights less than 100 feet." The author exhibited sketches showing the characteristic forms of the rocks, and concluded from a study of these that " ice had never prevailed along the entire western coast of Norway, neither had inland ice of any considerable thickness flowed over this coast in sufficient volume to wear off the points of the sharply fractured granite. Even the rocks below 100 feet are not more worn than is sometimes the case in tropical climates. The ' shark's teeth' of the Lofodens have not been planed down, nor is

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[3] Nature, xxxii. 180.   [4] Id. xxxii. 179.   [5] Id. xxx. 202-205.

there any vestige of the great ice-sheet of our text books within the Arctic circle upon the coast of Norway. Even in the fiords there is no evidence of ice action until we arrive at the head, where it is very evident. . . . The angular character of the low rocky island, in front of Odde, shows that it (i.e. the ice) cannot have advanced further." He concluded that "at no period within geologically recent, say Tertiary times, has ice extended much further than at present. Seeing that the morainic matter now in the valleys has been derived from the hills, there must, formerly, have been a greater extent of land above the snow line, and this would cause a former extension of glaciers without resort to any extraneous theory of change of climate. The great ice age has left no trace on the Norwegian western littoral."

In answer to a critic who suggested that more recent frosts may have roughened the surface, and thus cleared away the glacial traces, he says, "The glaciation is peculiarly distinct in the heads of the valleys, where the granite has retained the polished surface left by the glaciers. The extraordinary fact is that rocks of grey granite close to the sea should be so sharp as they are, if ice had gone over them—for instance, the so-called ' shark's teeth ' of the Lofoden islands." [6]

This remarkable evidence seems to me conclusive in regard to the theory of a Scandinavian ice-sheet having crossed the North Sea. It can be supplemented, however, from its western borders as well as from its north-eastern one.

I have already quoted Croll's statement about the extent to which the Scandinavian Mer de Glace extended westwards. In regard to the Faroe islands he was soon answered by his distinguished disciple, James Geikie.

Mr. Geikie frankly confesses that the great Mer de Glace did not reach the Faroe islands, " for, as Mr. Helland and I found during a recent visit, these islands supported a local and independent ice-sheet of their own, which flowed outwards in all directions into the surrounding ocean." [7] Again, he says, " Mr. Helland and I found that the Faroe islands . . . supported an ice-sheet of their own, the upper surface of which rose to a height in the northern islands of 1600 feet, and in

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[6] Op. cit. xliii. 83, 84.　　　　　[7] Prehistoric Europe, 205, 206.

Suderoe of 1400 feet above what is now the sea level . . . forming a compact mer de glace which flowed outwards in all directions from the dominant points, and discharged its icebergs into the surrounding ocean." [8]

This being the case with the Faroes, let us now turn to the Shetlands.

Mr. D. Milne Home, in protesting against the view that the Shetland islands were smothered in a mass of ice from Norway, in the Q. J. G. Soc. for November, 1879, says: " Even on the east coast of the Shetlands, where the striations on the rocks should show a north-east direction, there is no uniformity in that direction. Near the south end of the group, viz. at Bressay and Lerwick, as the arrows on the map show, the direction of the striæ is not from north-east to south-west, but from north-west to south-east; also at the north end of the group of islands, viz. in Unst."

Mr. C. W. Peach, in the British Association reports for 1864, says that ruts and striæ fell under his notice in north Unst on the cliff at Hagdale, in Haroldswick Bay; the direction being nearly west-north-west and east-south-east, and he adds, " that on ascending the Muckle Heog Hill, which reaches a height of at least 500 feet, I found the west-north-west end *vertical and polished to the depth of at least* 150 *feet.*"

Writing in *Nature*, in September, 1877, in regard to Mr. Peach's report, Mr. J. Geikie says he can speak confidently as to the correctness of his determinations.

In regard to the west coast of the islands, the markings are north-west and south-east, whence it has been inferred that the force which made them came from the south-east, but why not just as well from the north-west? It is not always easy at first sight to tell by the appearance of parallel striæ from what direction the striating agent had come. In the Lewis there are numberless marks on the smoothed rocks running north-west and south-east, which had been represented as indicating a movement from the south-east, " but on a minute examination of the striæ," says Mr. Milne Home, " I satisfied myself that the striating agent had moved from the north-west." [9]

[8] Prehistoric Europe, 504.

[9] See Royal Society of Edinburgh's 5th report.

Messrs. Peach and Home make the general striation of the Shetlands from the north-east, and that of the Orkneys from the south-east. " I assert," says Mr. Milne Home, " that this huge mer de glace, which they allege to have been at least 600 feet thick, above 200 miles wide, and coming from Scandinavia 400 miles distant, cannot have flowed in two directions, the one at right angles to the other."

Having laid it down that the Orkneys contain no mass of elevated ground which is capable of giving rise to a local ice-sheet, they afterwards postulate local causes as deflecting the great ice-sheets in certain places, and speak of the later, that is the lesser, glaciation as causing the great deflection at Pomona; what does this mean?

Again, in the Orkneys, which are low, there was nothing to impede the ice flow, and the striations should have a general direction, but instead of this, on almost every island the striæ show divergence, in one case even up to 74°, and on spots at no great distance from each other. Thus in the small island of Eda, it is stated that on the shore about a mile to the north of the rock of Skail, striæ were observed on an inclined rock face trending west and south, while on the cliff top, the direction is west-south-west, a discordance of 55°. In Pomona along the coast line from Island Bay to Houlon Head, the trend varies west 12° to 42° north and in one instance to west 32 south, a discordance of no less than 74°.

To explain a discordance of 40° in Westray, it is said the ice must have been slightly deflected, as it impinged on the east slopes, the *lower* portion moving in the direction of the north-east coast line while the higher strata streamed westwards over the hill tops to Noss Head. " I venture to think," says Mr. M. Home, " that such a theory requires no special refutation."

Turning to the mainland, Messrs. Robert Chambers and T. Jamieson examined the rock striæ of Caithness, and the latter published a map of his results. "The impression left upon me," he says, "by all I saw was, that the movement had been from the north-west to south-east, for where I observed any indication of a *stoss seite* it was on the north-west side. In regard to Sutherland, Mr. R. Chambers found on the hills near the west coast that the striæ pointed to a movement from the north-west and

west-north-west. So at Rheconish on the west coast, at Lough Laxford, and the tract between Loch Eribol and Tongue Bay. Sir Roderick Murchison, in the Trans. Geol. Soc. for 1828, had long before said that " the highest hills in the Brora district afford upon their sides and summits distinct traces of a strong diluvial current, which has swept them free of covering matter, and deposited in the plain of Clyne Milltown a mass composed of the *débris* of the denuded hills. A large portion of the turf having been recently removed, the surface of the rock was seen to be scored with parallel lines ; the direction of the markings is uniformly from north-north-west to south-south-east."

Mr. Peach, junr., communicated to Dr. Croll the following : " Near the Ord of Caithness and on to Berridale (i.e. on the coast) the striæ pass off the land and out to sea, but near Dunheath, six miles north-east of Berridale, they begin to creep up out of the sea on to the land." [1]

Messrs. Peach and Horne explain the sandstone blocks from Eda found on Westray as carried thither by the Scandinavian ice-sheet, which must have overtopped the hills in its north-west march. If so, how came the blocks of granite and quartz, etc., there ? These stones do not occur in Eda. The only island where they occur is Pomona, and if they came from there it was from the south-south-west, quite in the opposite direction to Scandinavia.

Again, in regard to the great Saville boulder on Sanda Island. Dr. Patrick Niell argued it came from Stromness, while Professor Heskell says the only similar granite is to be found in Sutherlandshire ; if so, it could not have been carried by the Scandinavian ice-sheet.

Neither the rock striations nor the transport of boulders are in one determinate direction. The directions are most divergent.

Jamieson points out that the occurrence of beds of chalk flints along the crests of low hills running for six or seven miles inland from Peterhead, is inconsistent with the country having been swept over by foreign ice, which must have swept them away since they occur on the very top of bare, exposed

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[1] Climate and Tran. 453.

hills at heights of from 250 to 370 feet. They lie thickly on the ground for a distance of several miles, and their quantity and the extent of ground they cover forbid our supposing that they have been drifted from some foreign region.[2]

This being the evidence of the western lands as to a Scandinavian ice-sheet in the North Sea, it will be seen to correspond precisely with that of the Lofodens and the coast of Norway. It may be completed by another class of testimony altogether, namely the fact that both on the Scandinavian coasts and those of the British Isles we have mixed with the glacial clays and sands and sometimes piled up in considerable heaps, remains of shells, in some cases more or less whole and in others broken, which, so far as we can ascertain, lived contemporaneously with the deposition of these beds and were contemporary with the so-called glacial period. These molluscs cannot have lived in an area blocked up with solid ice, they must have lived in water. Nor again do they, even those which are most glacial, point to conditions such as are postulated by an extremely rigorous cold in the area of the North Sea. This I tried to show long ago in a paper I published in the *Geological Magazine*, from which I will quote.

Forbes professes to tabulate the results of the examination of the so-called glacial marine drifts up to the moment when he wrote, as they occur in North Wales, Cheshire, Lancashire, the Isle of Man, the Valley of the Clyde, and Caithness, and he tells us the number of shells known up to that time was about 124. "With few exceptions," he adds, "they are all forms now existing in the British seas." It is most pertinent and interesting to quote his tabulated results of the examination of these shells. He divides them into six classes.

First, species now living throughout the Celtic region in common with the Northern seas, and scarcely ranging south of the British seas. Of these he enumerates twenty-five species. Secondly, species which range far south into the Lusitanian and Mediterranean regions, but which are most prolific in the Celtic and Northern seas. Of these he enumerates twenty-four kinds. Thirdly, species still existing in the British seas, but confined to the northern portion of

[2] Q. J. G. S. xxx. 317.

them, and mostly increasing in abundance of individuals as they approach the Arctic circle. Of these he gives fourteen. Fourthly, species now known, living, only in European seas north of Britain, or in the seas of Greenland or Boreal America. Of these he gives sixteen species. Fifthly, species not now known existing, and unknown fossil in previous deposits, six species. Sixthly, species, fossil in the Coralline or Red Crag, but still existing in the South-European, though not in the British seas. Seventhly, extinct species, fossil also in the Crag.

If we exclude classes 5, 6, and 7, as containing unknown factors, what is the result of this tabulation ? That sixty-three species are found still living in the British seas in our very temperate climate, and only sixteen are confined to the European seas, north of Britain, or the seas of Greenland and Boreal America. Surely this is suspicious evidence upon which to found a postulate that these shells lived under glacial conditions only. But let us examine these sixteen species somewhat more closely.

I. Mr. J. Gwyn Jeffreys gives *Astarte multi-costata* as a mere synonym for *Astarte compressa*, which shell in its typical form he tells us occurs off Scarborough and off Mull, while of its three varieties, *A. Globosa* is common off Skye, and the coasts of Argyle and Ross and East of Shetland ; *A. striata* is plentiful on the coasts of Yorkshire, Northumberland and Durham, the Firth of Forth, the West of Scotland, and the outer Hebrides ; while *A. nana* is found fifteen miles N.W. of Mull. And of the species in general he says : " *A. compressa* and particularly the variety *striata* is extensively distributed throughout the North Atlantic from Spitzbergen and the White Sea to the Cattegat, as well as on the north-eastern coasts of America." [3]

II. *Leda rostrata.*—Forbes gives this as a synonym of the *Nucula oblonga* of Brown,[4] which Jeffreys again says is the same as the *Leda pernula* of Müller, and tells us he dredged a young live specimen and a small single valve of the shell in eighty fathoms off the Shetland coast; single valves not

[3] British Conchology, vol. ii. p. 316. Risso has recorded it, under the name of *Cyprina Montagui* as fossil in the " terrains diluviens " at Nice. [4] Op. cit. p. 419.

living have also occurred in Loch Duich, in Ross-shire, and St. Magnus Bay, on the west coast of Shetland.[5]

III. *Leda hyperborea.*—Is not referred to in Forbes's detailed list of glacial shells, nor do I know what species is meant by it.

IV. *Tellina Grœnlandiœ.*—Sars deems it a mere variety of *Tellina balthica,* whose wide distribution over the seas of Northern Europe is well known. The variety *Grœnlandica* is now found living in the Gulf of St. Lawrence.[6]

V. *Tellina calcaria.*—Jeffreys says this shell is found on the Danish coasts of the Baltic, and northward as far as Spitzbergen, as well as on the shores of Asia and America, from Bering's Straits to Massachusetts.[7] In a later volume he adds that a fine perfect and fresh but dead specimen of it was dredged by the late Dr. Möller off Fan Isle, between the Orkneys and Shetlands, while he himself got a valve in the same condition on the west coast of Scotland.[8]

VI. *Mya truncata,* var. β *Uddevallensis.*—This is the more northern variety of the ordinary *Mya truncata.* The normal form has been dredged on the Dogger Bank and in Shetland and the Bay of Biscay. I am not aware that the variety *Uddevallensis* has occurred in our seas, but Jeffreys says he dredged an intermediate form in Dourievoe in Shetland.[9]

VII. *Saxicava sulcata.*—There is much doubt whether this species be now living at all.[1]

VIII. *Pecten Islandicus.*—This shell is found living on the Norwegian coast from Finmark to Bergen, and has been dredged in a semi-fossil state in the Gulf of Naples,[2] where assuredly a Glacial Sea, properly so called, is most improbable.

IX. *Terebratula psittacea.*—This is the *Rhynchonella psittacea* of other writters. It occurs living in Finmark as far south as Tromsö, but valves of it have been on several occasions dredged in the British seas.[3] Forbes gives it as from the seas of Newfoundland, Labrador, Greenland and Norway.

[5] Jeffreys, op. cit. vol. ii. p. 158, and vol. v. pp. 173 and 174.
[6] Forbes, op. cit. p. 411.   [7] Op. cit. vol. ii. p. 390.
[8] Id. vol. v. p. 187.   [9] Op. cit. vol. iii. p. 70.
[1] See Forbes, op. cit. p. 410.   Jeffreys, vol. v. p. 166.
[3] Jeffreys, vol. ii. p. 23, vol. v. p. 164 ; Forbes, op. cit. p. 406.

X. *Fusus cinereus.*—This shell has been described from the coasts of the United States.[4]

XI. *Fusus scalariformis.*—A synonym for *Trophon chlathratus.* Jeffreys tells us it inhabits Spitzbergen, Iceland, Norway and the Faroe Islands, and the coasts of Northern Asia southwards to Japan, as well as Greenland and the Eastern and Western coasts of North America.[5]

XII. *Fusus Fabricii*—Jeffreys only gives Greenland and the White Sea as the habitat of this shell.[6]

XIII. *Littorina expansa.*—This is apparently the *Turbo expansus* of other writers, and the only habitat I can find for it is that given by Forbes as Arctic Seas, and coasts of America.

XIV. *Margarita inflata.*—The *Turbo inflatus* and *Turbo Grœnlandicus* of other writers. It lives, says Jeffreys, in every part of the Arctic Ocean, and on the coasts of the White Sea, Scandinavia, Iceland, Canada, and the States of Maine and Massachusetts, and he mentions also its occurrence as local, but not uncommon, about the West of Scotland, the Orkneys and Shetlands, in Dunnet Bay, Caithness, etc.[7]

XV. *Velutina undata.*—This shell occurs in the seas of Boreal America.[8]

XVI. *Natica clausa.*—This shell occurs in Scandinavia as far south as the Christianiafiord.[9]

This examination is assuredly very impressive. Of the various shells enumerated by Forbes and others as proving that glacial conditions prevailed when they lived, there is hardly one which is not still living in the North Atlantic at the present moment under conditions completely different to those we describe as glacial. No doubt some of these shells do live and thrive in very high latitudes, and doubtless also have their focus there at this moment; but it is no less true that they extend down into the temperate regions of our own seas and those of the United States and of Southern Scandinavia. If the whole of the mollusca of the beds we are dealing with belonged to the same facies, we should be bound no doubt to

[4] Forbes, op. cit. p. 425. [5] Jeffreys, op. cit. vol. iv. p. 321.
[6] Op. cit. vol. iv. p. 322. [7] Op. cit. vol. iii. p. 299.
[8] Forbes, p. 422.
[9] Jeffreys, vol. iv. p. 229. Dr. Van Geuns discovered it in a Pliocene bed near Palermo.

conclude that the conditions of South Greenland or of Iceland prevailed here when these shells were living ; but the fact is this class forms a small proportion of the whole. Here we must at once say that Forbes's method of tabulating the results of his examination of all the beds together will no longer be deemed satisfactory. We have now the clearest evidence, both in Scandinavia and in Britain, that some of the beds have a much more arctic facies than others. We have in both areas beds which apparently represent the old sea-bottom of mud or sand with the shells still remaining *in situ*, bivalves standing erect with both valves intact, and with all the appearance of being undisturbed. These are the lower beds. For the most part, so far as I know, they occur at low levels and are characterized largely by shells of an arctic type.

Another series of beds, comprising those which are raised a considerable height above the sea, contain a congeries of mixed shells, often broken, and clearly, as we shall show presently, not *in situ ;* sometimes the shells are imbedded in sand and sometimes in clay. They contain generally a larger proportion of species, and belong to a more temperate condition of things. The fact that Arctic shells are found in these higher beds is undoubted ; but, as I contend, the meaning of this is very different to what is often supposed—the proportion of purely Arctic shells being by no means large, and under any circumstances the mollusca clearly pointing to a condition of things very different indeed to that which prevails in the Arctic seas. We must remember that not only is the number of purely Arctic shells in these beds as compared with the whole number comparatively small, but that the fact of finding a few distinctly Arctic shells is balanced by the occurrence in certain of the beds of another class which tells an entirely different story, namely shells characteristic of a more southern latitude than our own, southern forms which occur together with these Arctic forms.

In the thirtieth volume of the Journal of the Geological Society, Mr. Mellard Reade describes the marine shells from the Lancashire drifts in the neighbourhood of Liverpool and Warrington. In this paper, pp. 34 and 35, he says, " Though all, with the exception of *Astarte borealis, Leda pernula,* and

Saxicava norvegica (which are of course northern shells), may be described as Irish sea shells, yet, if we make a fair comparison, we shall find that the boulder-clay assemblage possesses a more northern facies than the present fauna of the British seas." Notwithstanding this sentence, Mr. Reade very frankly points to the presence of *Venus Chione, Dentalium tarentinum* and *Cardium tuberculatum*, as distinctly southern shells, and adds, " The presence of *Venus Chione* in the low level boulder-clays as well as in the Macclesfield drift, where it was first discovered by Mr. R. Darbishire, is certainly a remarkable fact; but it is not an isolated one ; for *Cardium aculeatum* and *Cardium pygmæum*, both Lusitanian forms, are found in the Scotch drift, and in the Irish drift southern forms also occur," adding in a note, " Forbes notes the discovery by Captain James of *Turritella incrassata*, a Crag fossil, a southern form of *Fusus*, and a *Mitra* allied to the Spanish species in the Wexford gravels."

In a paper by Mr. R. Darbishire, on the Shell-drift at Leyland, which was published in the same volume of the Geological Journal, the author says, " The list, although containing a few names of species of a northern character (*Astarte* and *Fusus*), contains also several shells of markedly southern origin. *Venus* (*Cytherea*) *Chione*, which has been identified at Macclesfield, and in several of Mr. Reade's Liverpool localities, may almost be called a characteristic fossil at Leyland. *Cardium tuberculatum* (*rusticum*, F. and H.) occurs not unfrequently. One perfect and characteristic hinge of *Mactra glauca* (*helvacea*, F. and H.) was found. All these are essentially southern species. The only shell which is peculiarly Arctic in character is *Fusus* (*Trophon*) *craticulatus*, Fabr., a species now living in Greenland, of which one fine and (for drift) fairly fresh-looking shell has been found. The same species was identified at Moel Tryfaen." [1] The presence of these southern forms is surely just as eloquent and just as forcible evidence as that of the northern species, and when these two papers were discussed, Mr. G. Jeffreys said, " All the shells found in these Lancashire beds were just such as might have been thrown up on the shore, though the matrix

[1] Op. cit. p. 39.

in which some of them are found is not sandy. . . . He did not regard any of the shells as truly arctic, and doubted whether any of them afforded clear evidence of climate." Mr. Prestwich said, " In the overlying boulder-clay the fragments of shells were all of species still existing in the neighbouring seas of the present day; and he did not think that at the time of its deposit the climate was of necessity intensely cold." While Prof. Hughes was so much struck with this that he submitted that the shells belonged to an age succeeding the true glacial period.[2]

Mr. Darbishire says of the drift shells from Leyland, with the exception of the *Fusus (Trophon) craticulatus*, Fabr., " The series from Leyland must be described as very similar to that of the fauna of the present seas along the western shores of Britain."[3] In a paper on the drift deposits found near Blackpool, by the late Mr. Binney, published in the 10th volume of the Memoirs of the Manc. Lit. and Phil. Soc. p. 127, etc., he has described the shells found in the till there, to the number of nineteen, all of them being shells still found on the Lancashire coast. A similar assortment of shells was found by Mr. Harkness in the till near Ormskirk.[4] These shells assuredly witness no glacial climate. A more remarkable find, because of its locality, fifty miles from the sea, 568 feet above the sea-level, and right up in the gorges of the English Appenines, which, in the glacial period, must have been choked with permanent ice, if the glacier theory, as usually taught, is sustainable, were found by Mr. Bateman, the engineer, in making the reservoir at Hollingworth, in Mottram in Longdendale. These included *Turritella terebra, Fusus Bamfius, Purpura lapillus,* two species of *Tellina* and *Cardium edule.*[5]

Let us now turn to the famous beds found near Maccles-field, and which occur there as much as 1200 feet above the sea-level. On these shells Mr. Darbishire wrote a valuable paper in the third volume of the third series of the Memoirs of the Lit. and Phil. Soc. of Manchester.[6]

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[2] Op. cit. p. 41.　　　　　　　　　[3] Journ. Geol. Soc. vol. xxx. p. 39.
[4] Id. p. 130.
[5] Proceedings Lit. and Phil. Soc. of Manchester, vol. iii. pp. 15, 16.
[6] See also Geol. Mag. 1865, vol. ii. p. 293.

Mr. Darbishire describes forty-nine shells from this locality, all except seven occurring in Forbes's list already quoted.

Of the forty-two named by Forbes, four reach their limit within the British seas, six extend southwards as far as the British Channel, while " of the remaining thirty-two species, the whole now range considerably southward of the British Isles, but, as a set, present a characteristically British aspect." The seven shells not named by Forbes are *Pholas candida, Cytherea Chione, Cardium rusticum, Cardium aculeatum* (?), *Arca lactea, Littorina littoralis,* and *Dentalium abyssorum.* None of these appear, says Mr. Darbishire, " in McAndrew's list of mollusca, observed between Drontheim and the North Cape, and only one, *Littorina littoralis,* in Danielsen's Zoological Notes of the Scandinavian coast. The remaining six are all shells of species which at present reach their *northern limit* within the British seas, extending to our western shores from the Spanish province. *Cytherea Chione, Cardium rusti-cum, Cardium aculeatum,* and *Arca lactea* are characteristic shells of a Spanish or southern type. The *Cytherea* is not found north of Carnarvon Bay nor in the German Ocean, it is essentially a southern species. The *Cardia* rarely frequent the coasts of Devonshire and Cornwall. *Cardium aculeatum* is said to have been dredged off Bergen ; but *Cardium rusti-cum* is not known east of the Channel." [7] Mr. Sainter says of the two *Cardia, Cardium rusticum* is also essentially southern and Spanish, Bantry Bay being now its most northerly and authentic distribution. *Cardium aculeatum* reaches its highest northern range in the south or west of England and Ireland. *Arca lactea* is also a southern shell, ranging northwards as far as Berwick Bay and Oban. [8]

Not far from the deposits just described, but at a level 600 feet higher, Mr. Prestwich found fragments of shells. This spot is situated near the Buxton New Road, about a mile eastward of the first toll-bar out of Macclesfield, and, according to Mr. Darbishire, is from 1120 to 1160 feet above the sea-level. Here the latter gentleman found fragments of twelve species of shells, namely, *Psammodia ferroensis, Tellina solidula, Mactra, Cytherea Chione* (a characteristic hinge-

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[7] Op. cit. p. 62.   [8] Scientific Rambles about Macclesfield, pp. 57, 58.

fragment and another), *Artemis lincta, Astarte arctica, Cardium echinatum, Cardium edule, Mytilus, Turritella communis, Fusus antiquus,* and *Trophon.* After enumerating these shells, Mr. Darbishire adds the striking comment, " The occurrence of the *Cytherea* in this bed at a height of 60° feet above the beds examined on the west of Macclesfield is very curious, and adds a formidable consideration to the many difficulties which seem as yet to delay the solution of the ' Drift ' problem." [9]

If we turn from these high beds in Cheshire, to the famous beds at Moel Tryfaen, in Carnarvonshire, we shall find that, although the Lusitanian species which are found at Maccles-field are absent, the general facies of the deposit is the same. Of fifty species found there, as many as thirty-seven are still found living in the Irish Sea, while eleven only are typical of a more northern habitat. The evidence on the whole pointing no doubt to a colder sea, such as is known on the Scandinavian coasts and those of Boreal America, but in no way justifying the notion that it was like that of the Polar regions.

The Irish drift-beds were treated in a compendious paper by Mr. Bell in the tenth volume of the *Geological Magazine.* The evidence they furnish is like that of the beds on the opposite side of St. George's Channel. In the well-known Wexford beds sixty-nine species of shells have occurred, of which as many as fifty-two still live in the British seas. Ten, that is, barely one-seventh, are northern forms, viz. *Astarte borealis, Leda hyperborea, Leda oblonga, Nucula proxima, Natica affinis, Pleurotoma Vahlii,* (?) *Scalaria Grœnlandica, Trophon Fabricii, T. clathratus, Volumitra,* sp. *Grœnlandica.* These northern forms, however, are balanced by the occurrence with them of the southern forms *Leda pusio* (?) *Fusus crispus* (?) *Nassa semistriata, Turritella incrassata,* one Japanese species, *Nucula Cobboldiæ,* and two others whose habitat is unknown, *Melampus pyramidalis* and *Fusus Bailyi.*[1] In the Dublin drifts thirty-three species have occurred, of which twenty-nine still live in our waters. Of the other four, two, namely, *Loripes divaricatus* and *Woodia digitaria,* are Lusitanian forms, and found on the East Atlantic coasts of Spain.[2]

On the banks of the Logan and the southern shores of

[9] Id. pp. 64, 65. [1] Op. cit. pp. 451, 452.
[2] Jeffreys' British Conchology, vol. v. p. 179.

Belfast Lough thirty-seven species of shells have been found in the drift clays by Messrs. Bryce and Hyndman. Of these, only two, *Leda oblonga* and *Trophon clathratum*, are not now found in the British seas, but only in the seas further north.[3]

Mr. Bell has also shown that a bank of shells, now submerged, off the mouth of Belfast Lough, and known as the Turbot Bank, consists largely of drift shells. Speaking of these latter shells he says, " Amongst the species, all of which are small, *intermixed with Celtic* and *southern forms*, are nine or ten whose congeners now live in the northern seas," adding, " The intermixture of southern forms with those of higher latitudes does not interfere with the placing of these beds in the Post-Tertiary series, since they are equally present in the Killiney drifts, the Lancashire drifts, and in some of the Scottish Clyde beds, especially the one near Greenock. Indeed, amongst a parcel of minute shells and shelly clay from the latter place, I detected the fry or extreme young of some living Mediterranean forms, *Conus Mediterraneus* and *Cardita trapezia*."[4]

Let us now go beyond our own land. " At Lillehersstehagen, which lies about an English mile east of Uddevalla, in Sweden, an extensive deposit of shells is partially exposed. Here the examination gives a singular result. Mixed with the universal *Trophon clathratus* (which is a high northern species, and found living only within the arctic circle), are many shells of rather a southern type, such as *Ostrea edulis, Tapes pullastra, Corbula gibba*, and *Aporrkais pes-pelicani*. All these species, however, have been recorded by Sars as inhabiting the coast of Finmark, although they are also natives of the Mediterranean. According to Dr. Torell, a living oyster has never been found in the seas of North Greenland and Spitzbergen."[5] This deposit is paralleled in Norway, where southern forms of shells have occurred mixed with northern ones in the raised beaches.

No doubt both in Norway and Sweden we find raised beds of shells showing different climatic conditions, some presenting a more Arctic facies than others, but all of them prove the

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[3] Geol. Mag. vol. x. p. 449.   [4] Id. p. 450.
[5] Jeffreys' Report on Upper Tertiary fossils of Sweden, Brit. Assoc. Report, 1863, 1874, and 1875.

existence of open water and not of ice-bound coasts when they were deposited; but even the difference of the contents of the beds may have its lessons exaggerated. " It is true that in some districts the Post-Tertiary fauna differs in one spot from that of another, at no great distance, in its apparently more northerly or southerly aspects, which unquestionably might arise from local causes, independently of climatal changes. The same thing holds good at the present time. For instance, *Fusus Turtoni*, *Fusus Norvegicus*, *Fusus Berniciensis*, and *Saxicava* (*Panopæa*) *Norvegica*, all northern species, have been taken living on the coasts of Durham and Northumberland, while no trace of them has been found on the western coasts of Scotland. We may reasonably infer that similar variations occurred in the seas of ancient times, yet no one would ascribe them to a change of climate." [6]

Messrs. Crosskey and Robertson most aptly say : " A species may be rare at one part of a clay pit, and within a hundred yards or less, and in precisely the same clay, be abundant and characteristic, so that a fresh excavation may give a new aspect to the apparent grouping of the various forms. Exactly the same method of distribution prevails with these fossils from Post-Tertiary beds as with the inhabitants of the sea-bottom of the present day. Different species have their peculiar individualities of habitat, often within very short distances of each other. *Solen ensis*, e.g. is plentiful round Cumbrae in all the banks of muddy sand, but upon one bank *S. siliqua* occurs, and there scarcely an example of *S. ensis* has yet been found, although the banks are as nearly as possible similar in composition and not a quarter of a mile from each other. In the same way, *Pecten Islandicus* is abundant in the glacial clay in one locality, and will be replaced in the immediate neighbourhood by a different characteristic shell. *Cytheridea punctillata* (to give another example)—an abundant fossil form—is plentiful living in Loch Fyne, but rare in every other part of the west coast. The collector of glacial fossils is thus in exactly the position of the marine naturalist—he has to deal with an elevated sea-

[6] Robertson, Notes on Raised Beach at Cumbrae, Trans. Geol. Soc. of Glasgow, vol. v. p. 193.

bottom in which many forms had their own especial dwelling-places."[7]

The fact is, the distribution of the mollusca is in all probability ruled by the same laws as that of the land fauna. It depends a great deal more on a sufficient supply of suitable food than on merely climatic conditions, and when we find southern and northern forms mixed together in so-called glacial beds, as they have been so clearly shown by Mr. Searles Wood to be mixed in a larger degree in the beds of the Crag, we ought to feel no more surprise than we do when we meet with the remains of the reindeer and the hippopotamus lying together in the brick-earths of the Thames Valley, or the reindeer and the tiger living together, at this moment, in Manchuria, and the camel and mountain-sheep in Tibet. This analogy points another moral. The two sets of marine and subaerial beds were contemporaneous. Of this we have remarkable evidence in certain situations where the *débris* of the land-life and of the sea-life have been mingled together. Thus in Western Sweden, in the black clays, the following list of shells marks the molluscan fauna : *Mytilus edulis, Mya truncata, Modiola modiolus, Solen ensis, Cyprina islandica, Nucula nucleus, Leda pernula, L. caudata, Cardium edule, C. echinatum, C. fasciatum, C. norvegicum, Lucina borealis, Montacuta bidentata, Isocardia cor, Pecten islandicus, P. maximus, P. septemradiatus, P. striatus, Saxicava rugosa, S. arctica, Tellina proxima, T. solidula, Astarte elliptica, A. sulcata, A. compressa, Thracia villosiuscula, Mactra subtruncata, Tapes pullastra, Venus striatula, V. ovata, Scrobicularia piperata, Ostrea edulis, Anomia patelliformis, A. aculeata, Littorina littorea, L. rudis, L. littoralis, Trochus cinerarius, Natica nitida, N. Montagui, Lacuna pallidula, L. vincta, Turritella communis, Cerithium reticulatum, C. adversum, Purpura lapillus, Nassa reticulata, N. pygmæa, Aporrhais pes-pelicani, Buccinum undatum, Fusus despectus, F. Turtoni, Trophon clathratus*, var. *minor, Mangelia linearis, Patella vulgata, Acmæa virginea, Lepeta cœca, Dentalium entale, Balanus porcatus, B. crenatus, B. Hameri.*[8] The collocation of these

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[7] Trans. Geol. Soc. of Glasgow, vol. ii. p. 269.

[8] Exposé des formations Quaternaires de la Suede, by A. Erdmann, pp. 92, 93.

forms is itself curious. The greater part of them still live on the coasts of Sweden, others now live further north, but the specimens from the beds are larger and finer than those now living in the same neighbourhood, proving that the conditions of life were then more favourable. Having examined the shells, we shall do well to turn to a notice, furnished by the author just quoted, of the remains of land plants found in the upper parts of this black clay, where exposed in a typical section at Enköping. Here, he says, is a quantity of vegetable *débris* of very different kinds, as, for example, stalks of *Equisetum limosum* mixed with fragments of conifers and of leaves of the oak, willow, and the aspen, leaves and cones of the pine, twigs, branches, and bark of the fir, the aspen, etc. With them are found the epidermous coverings of the *Mytilus edulis*, of which all traces of the shells have disappeared. These *débris* assuredly point a very important lesson. Oaks would not be growing, nor pine trees bearing cones, in contiguity with a glacial sea.

If we come nearer home, we have a remarkable example in the marine drift at Kelsea Hill, at the mouth of the Humber, described by Mr. Prestwich in the seventeenth volume of the Geological Journal. From these beds Mr. Prestwich collected forty species of shells. It is not necessary to enumerate them here, but I may add the conclusion arrived at by Mr. G. Jeffreys. He says, " Although nearly all of the species comprised in the foregoing list still exist in the German Ocean, there are some which are only known as living in more northern seas." Mr. Prestwich himself says, " The number of shells now obtained from these beds show, however, a more northern character than was at first apparent, and tend, therefore, taken in conjunction with the occasional capping of thin seems of clay like a boulder-clay, and the fact of a general development of gravels to the westward, incline me to believe that these beds are to be referred to the upper part of the boulder-clay. The shells are all of recent species, and thirty-three out of the forty are still found on the Yorkshire coast. Nevertheless the presence of such species as *Natica clausa, N. Grœnlandica, Trophon Gunneri, T. scalariforme, Mangelia pyramidalis* and *Littorina squalida,* which have a wide northern and Arctic range, indicates colder

conditions than those now prevailing on these shores, and more in accordance with what we know of the fauna of this portion of the Post-Pliocene series.[9] What is very curious, however, is to find this same northern fauna associated not with individual sporadic shells, but with thousands of examples of the freshwater shell *Cyrena fluminalis,* now confined to the Nile and the southern latitudes of Asia, and in all probability also associated with the mammoths' tusks which have been found in the estuary of the Humber; the *Cyrena* and the mammoth being companions in the Thames Valley and elsewhere. Again, during the construction of the Forth and Clyde Canal, some beds were met with at Croftamie (Drymen Station). In a bed of blue clay the following shells were found: *Cyprina islandica, Astarte compressa, Fusus antiquus, Littorina litorea,* and *Balanus,* and in the same bed was a *fragment of deer's horn eleven and a half inches* long, which was declared by Professor Owen to be that of a young or female reindeer of the existing species.[1] These shells, says Mr. Jack, indicate a climate not severe, though probably a little colder than the present climate of these latitudes. All the shells specifically distinguished are living in the British seas.[2] This deposit was more than 100 feet above the sea level. In other deposits near Loch Lomond, also described by Mr. Jack, and situated up to as high as from 120 to 262 feet above the sea-level, were beds of shells, which he describes as water-worn marine shells of species almost all now living in the British seas; the deposit, therefore, indicating a climate little more severe than our own.[3]

In concluding this part of my argument, I would strengthen it by two quotations from the opinions of Sir Roderick Murchison, based on his continental experience.

" But," he says, " some of the very marine shells on which we have been insisting as proofs of the aqueous formation of this boulder drift, are said to be Arctic species, and have therefore been quoted as indicating the prevalence of a colder climate in our latitudes in those days than at present. Hence, glaciers, it is supposed, may have been adjacent to such arctic

[9] Op. cit. pp. 455, 456.   [1] Trans. Geol. Soc. of Glasgow, vol. v. p. 16.
Id. p. 25.   Id.

animals. But what are the species of shells associated with the great boulder drift in Denmark? Why, in many tracts, the very same which now live *in the adjacent seas*. And though several of the latter are arctic species, no glaciers occur within several hundred miles of sea in which they live. Again, the researches of Prof. E. Forbes in the Ægean and of Prof. Loven in the North Sea have taught us, that the more or less arctic character of shells essentially depends upon the depth of the submarine zone at which the animals lived." [4]

Again, " We were at one time disposed to think that the presence of sub-fossil shells of arctic character naturally indicated the former presence of a much colder climate in those latitudes where they have been found; but, independent of discoveries in submarine life, we now hold that it is unnecessary to have recourse to such an argument, in relation to any phenomena in the British Isles or similar latitudes; for we can easily imagine, that when very different physical features prevailed, and when lands now above the sea were beneath it, cold currents may have extended very far southward of the arctic circle, and have been inhabited by species now restricted (through geographical changes) to a less horizontal range." [5]

What is the burden of all these facts? Why, assuredly that the shells found in our so-called glacier drifts, or at least in nearly all of them, far from bespeaking conditions of climate such as can alone be fairly described as glacial, on the contrary, speak to us of a time when the general temperature was perhaps somewhat lower than it is now, but when the North Sea and North Atlantic were filled with open water, and bathed a land where the mammoth and the rhinoceros could find abundant food, where the oak and the pine flourished, and where the rivers could sustain such molluscs as the *Cyrena fluminalis*. This conclusion destroys at once the basis of those who have argued that our glacial drifts containing shells were left where they are found by ice.

We are further justified in concluding from these facts that the notion that a great ice-sheet once filled the North Sea is

[4] Russia and Ural Mountains, i. 551.   [5] Id. note 2.

negatived by the character of the molluscs found in the drift beds. The ultra glacialists are not satisfied, however, with postulating this, they go further, and urge, as we have seen, that the shells were actually scooped out of the sea bottom by the ice-sheet, pushed up on to the land and deposited in the beds where we find them. How the great ice-sheet, which is supposed to have moved westwards, could at the same time have pushed shells up on the coasts of Scotland and of Scandinavia, in opposite directions, I don't know.

Again, we must remember that the semi-fossil littoral shells dredged by Jeffreys, in deep water off the Shetlands, show that the bed of the North Sea was not scoured in the way supposed. Lastly, if the North Sea was thus occupied with ice, where did the arctic molluscs migrate to? When we get into the English Channel, and the Irish Channel also, we find that the contemporary shells are not of arctic types, but contain a proportion of Lusitanian forms. Were the Arctic species exterminated, or did they go on living in a solid sea of frozen water?

Again, turning to another argument. It is incredible that Iceland and the archipelagoes north of Scotland have been united to the mainland *since* the distribution of the glacial drift. I know of no evidence whatever to support such a conclusion, while the evidence of the shell beds found in Scotland and elsewhere clearly point the other way. If so, how did these islands acquire their plants and animals? Are they all the descendants of casually drifted ancestors? Assuredly the position is not arguable even. If so, this forms in itself a strong piece of evidence against the conclusion in question, and we may take it that the evidence of a Scandinavian ice-sheet traversing the North Sea is irreconcilable with the facts.

Having examined the problem on the western side of Scandinavia, and shown that there is no evidence of its ice having moved westwards for more than a very short distance, let us turn to the countries surrounding the Baltic. As Pettersen says, " If the Norwegian ice, whether a glacier or an ice-sheet, was not strong enough to force its way to the islands along the coast, it would be remarkable if it could make its way right through the great trough of the Baltic and across

the Polish plains for 600 miles." As a matter of fact the evidence seems to me to be most complete that it did not do so. The provénance of the blocks found in Northern Germany has been much more carefully studied in recent years, and the result was embodied in some elaborate memoirs by Von Roemer, entitled "Lethæa Erratica." He has shown that there is no evidence of any movement of erratics from northwest to south-east, nor are any erratics from Norway found in the boulder clays of Germany at all.[6]

It is true that Norwegian blocks have found their way to Denmark, and are described by Deichman Brandt, but it seems impossible to understand how any ice-sheet could have taken them there, for if we examine Kierulf's very interesting map of the distribution of the Norwegian blocks in Norway itself, we shall find that all along the southern coast of Norway the blocks from the Christiania Sound have been carried right round the coast in a direction athwart that of any ice-sheet moving from Norway southwards.

The same map is very instructive if we turn to the eastern flanks of the Dovrefelds. South of the latitude of 62° we shall find that the erratics have moved from the central mountains outwards in an easterly and south-easterly direction, but north of this parallel and as high as 65° they have moved in the reverse direction from south-east to north-west; further north they have moved directly west. This is assuredly most inconsistent with any ice-sheet theory, and points to the movements having been locally controlled by the contour of the ground or otherwise. The direction of the striæ as given in a second map agrees with that of the blocks.

In regard to the difficulties attending the distribution of the erratics of North Germany, Mr. Geikie frankly states some of them. At Lyck in East Prussia, at Trebnitz and Steinau in Silesia, at Meseritz in Posen, and at Berlin, we find fragments of Silurian rocks which are recognized as having come from the island of Gothland in the Baltic. All these might quite well have been rolled forward under one and the same ice-sheet, but how are we to account for the presence in the boulder clay of Gröningen in Holland of boulders of the same

rocks? It is evident that these last must have come down the basin of the Baltic and crossed the route followed by the others nearly at right angles. Nor are these cases altogether exceptional, for we learn that erratics from Esthonia have been detected in boulder clay at Hamburg, and that fragments derived from the island of Oland are met with in the till of Faxö in Denmark.[7]

This is perfectly true and confirmed by the best observers. Thus Von Roemer says of the North German erratics, "While a considerable number have come from Sweden, another portion have come from the eastern parts of the Baltic. The Silurian and Cambrian erratics have come from both these areas. Boulders of Devonian age are chiefly found in the eastern provinces of Prussia. They have come, undoubtedly, from Livonia and Courland. The Jurassic boulders found as far west as the Elbe are most nearly allied to the Jurassic strata of Courland. The Cretaceous boulders have come from Sweden or Denmark and occur in the whole area."

Helland did not find among the boulders of Holland any that had come from Scandinavia, nor are there, in the collection at Leyden, any blocks containing fossils which came from the same peninsula. A block with *Paradoxides Tessini* he derives from Oland. Other blocks in Holland came from Gothland, and the greatest number from Esthonia. This accords with the view generally held that the Dutch erratics came chiefly from Oland and Esthonia.[8]

A striking case is presented by Professor Martin of Leyden in his notes on the erratics of Overyssel, of which a collection is preserved in the museum at Zwolle. They include granite and gneiss, probably from Sweden; limestone with characteristic fossils from Gothland and the Russian Baltic provinces; Devonian spirifer-sandstone (very abundant from the Rhine district near Coblenz); carboniferous limestone from Belgium near Aix-la-Chapelle; a specimen of *Goniatites sphericus*, probably from the valley of the Ruhr; Trias with *Ceratites nodosus* from North-west Germany, Lias also probably from the same place; Dogger, etc.,

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[7] Prehistoric Man, 564.

[8] Martin Ueber das Vorkommen Eines gem. diluvium, etc., in der Dammer Berger in Suden Oldenburgs, note 330.

from the Rhine districts; flints with chalk fossils from North Germany and near Aix-la-Chapelle; Tertiary fossil wood from Upper Cassel near Bonn; Miocene " sharks' teeth " from Belgium and the Netherlands, and lastly, agates and pieces of chalcedony from the Rhine district. These erratics clearly show that they have been derived partly from the north-east, embracing the districts bordering the Baltic; partly from the south, from the Rhine valley and its tributaries; and partly from native rocks which underlie the present surface of Overyssel and the adjoining provinces.[9] It seems clear that the force which distributed these boulders was one capable of bringing some of them from the north to the south, and others from north-east to the south-west. How could an ice-sheet culminating in Sweden or Finland, and pouring over the Baltic, possibly distribute the boulders in this fashion ? Nor do the boulders stand alone. The glacial striæ are equally inconsistent with the theory we are criticizing.

" In the north of Norway and Sweden and in Lapland," says Jamieson, " the whole country seems to be ice-worn from the mountain-tops down to the sea. The scores radiate from the central heights to all points of the compass. Along the coast of Norway they run to the west and north-west; in Lapland to the north and north-east; in Sweden to the east and south-east. Between 62 and 63½ north lat. however, the erosive agent proceeded out of the relatively low ground of Sweden and marched *up-hill* right over the Dovrefeld." " Sans exception," said Horbye, " toutes les stries qui se trouvent sur la frontière mentionnée entre le 62ième et 63½ dégré de latitude ont leur point de depart dans les contrées de la Suede relativement plus bas." [1]

This last fact is very curious, and seems to be paralleled by what we have in America, where, according to Sir William Dawson, the force which moved the boulders must in many instances have moved from the sea, landwards. Reverting to Scandinavia, we cannot avoid the general conclusion that if the Dovrefelds, with their great heights, could not impel the ice upon them beyond the edges of the mountain range, neither could the comparatively flat lands of Sweden and Finland do

[9] Geol. Mag. 1884, p. 462.
[1] Horbye sur les Phénomènes d'érosion en Norvége, p. 110. Jamieson, Q. J. G. S. xviii. 181.

so ? The facts we are considering are not limited to the north of the Baltic.

Along the shore between Kalmar and Karlskrona, Murchison and Horbye found the striæ were parallel to the shore. At Upsala and Stockholm the grooves are north and south. The northern banks of Lake Malar are rough, while the south banks are smoothed and rounded. The Aland islands are also striated from north to south. At Gothenburg, and all round lakes Wener and Wetter, the striæ are from north-east to south-west or south to west.[2]

Mr. T. Tegraeus gave an account of his studies of the glacial formations of the Isle of Gothland, before the Geological Society of Stockholm, on March 4th, 1886. He stated that blocks and drifts proved that the glaciers had first moved in a south-west, and afterwards in an easterly direction.[3] All this shows how inconsistent the facts are with an overwhelming Scandinavian ice-sheet.

I have already pointed out in the last chapter some of the difficulties involved in the theory of an ice-sheet, when we have to explain the local and aberrant distribution of the boulders in Germany, etc. Another important feature which strongly militates against the theory, is the fact that the boulders on the continent generally occur in the valleys, whilst the watersheds are free from them. Thus, Murchison says, " there is indeed no feature more curious in the distribution of the drift along its southern frontier, than its far advance to the south along certain great valleys, and its omission in such localities upon the interjacent higher grounds. Such, for example, are the heaps of detritus, syenite, granite and greenstone which advance into the valley of the Don near Voronej, and on the west by another north and south route, parallel to the neighbourhood of Putievil on the Sem, a tributary of the Desna, leaving the central dome of Orel almost, if not entirely, free from such transported matter. These are truly remarkable facts ; and we feel confident that there are no other parts of Europe in which foreign materials have been transported so far as from Russian Lapland, and Finland to Voronej and Putievil, points from 700 to 800 English miles in straight lines from

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[2] Climate and Time, 446, etc.   [3] Nature, xxxiv. 164.

the nearest crystalline rocks, whence such fragments can have proceeded. . . . In Poland and in the adjacent part of Russia, we found that in the great valleys of the Vistula and the Oder, the blocks were distributed precisely as in the valleys of the Don and the Desna. Along the Vistula they range in rare and isolated specimens up to the environs of Crakow (500 miles from the nearest shores of Sweden), where the northern granites are easily distinguished by their character from those of the adjacent Carpathian chain, the fragments of which, like those of many other mountains, never advance more than a few miles beyond its flanks. In the valley of the Oder also, the same northern materials, quite distinct from those of the flanking Silesian mountains, are found in great abundance at Gleinitz, where they are extended from the chief boulder country north of Breslau, in a long *trainée* between the chain of Silesian crystalline rocks on the west, and the hilly districts of Poland on the east. . . . A considerable portion of Southern Poland, particularly all that tract of palæozoic rocks of which Kielce is the centre, is thus peninsulated, like the dome of Orel in Russia, between two advanced lines of boulders, none of which have been distributed in the intervening higher space."

Again, if we postulate that the boulders of Germany, etc., were transported by an ice-sheet which must have filled up the Baltic, how are we to explain the presence of the sea-shells in the drifts bordering that sea, in the Swedish asars and shell beds on the one hand, and in those of Prussia and Pomerania on the other? These shells agree with those from the borders of the North Sea, in that they do not point to an arctic climate having then existed in the Baltic, nor could they have lived except in water. Of the presence of water and not ice we have evidence, not only in the shells, but in the stratified drift on both sides of the Baltic. Stratified beds cannot be deposited by an ice-sheet. It will not do to say that the shells lived before the glacial times, for the shell beds lie on the polished and glaciated surfaces, and are themselves overlaid by boulders in many cases.

All this evidence seems to me to be conclusive against the drift of Russia and Germany having been deposited by an ice-sheet coming from the north, and this means against its

having been deposited by land-ice in any shape, for I know of no other way in which it could have been distributed by land-ice, except by such an ice-sheet. If we are to explain, therefore, the distribution of the northern drift in Russia and Germany, and the occurrence of northern erratics in Eastern England, we must have recourse to some other force than that of ice.

What the evidence points to, is a large development of local glaciers in the mountains of Norway, and perhaps in the wide rolling districts of Lapland and Finland, whose surface resembles that of the islands in the American arctic archipelago. This, so far as we know, was coincident with a climate on the plains and lower country by no means remarkable for extreme rigour. The local glaciers were probably the foster-mothers of many of the erratics which have been traced so far from their homes, but it seems to me absolutely clear *that the distribution of these erratics, and the explanation of the associated phenomena over the wide plain where they occur* must be sought in some other, and some subsequent cause, whose gigantic handiwork has been overlooked by those who have so largely magnified the power and capacity of ice.

Let us now turn elsewhere, and first to the Alpine district. In the Alps we have evidence pointing to precisely the same conclusion. In the first place, we have the double difficulty presented by the Scandinavian example. If the Alps were covered with an ice-sheet, sufficiently large to enable it to distribute the glacial *débris* of the Rhone valley, and the Alpine *débris* found on the Jura, it must have so smothered the whole mountain chain that we cannot well see whence the erratics could be derived. On the other hand, the evidence of the higher mountain heights in the Alps is conclusive against their having been covered by ice. This was admitted by Agassiz himself, the great champion of an Alpine ice-sheet.

In a paper by Desor in the Comptes Rendus for 1842 he says, " Pendant le séjour que nous fîmes sur le glacier de l'Aar, au mois d'Aout 1841, M. Agassiz, et moi, nous crûmes remarquer que les surfaces moutonneés, arrondiés et sillonnées ne dépassaient pas une certaine limite, tandis qu'au dessous de

cette limite la roche était ordinarement délitée, éboulée et hérissée de vives arêtes."

The rocks about the Grimsel point the same lesson. M. Labauer, who published a memoir on the fight on the Col of the Grimsel in 1799, speaks of these *roches moutonnées* as " sections de cylindre," and describes them as polished, while above, the rocks are all angular. The contrast must be remarkable to have arrested the attention of a man who was not a professed geologist. Desor says that he and Agassiz noticed the same thing on the Sidelhorn. The upper limit of the *roches moutonnées* is well defined and regular. Above the line all the pics rise from the midst of the snows, and are very broken and angular. M. Escher von der Luith noticed the same phenomena in the Pennine Alps.[4]

In another paper, Desor urges that above a limit of 2800 to 2900 metres, all the Alpine summits are angular and jagged, and their flanks are free from erratic blocks and *débris*. This is equally true of the gneiss mountains, such as the Schreckhorn, the Finster Aarhorn, the Munk, the Jung Frau, etc., and of the granitic mountains as the Rothhorn, the peaks of the Mieselen, the Therberg, the Zaesenberg, the Grüneberg, etc. When we descend to a lower level than that above named, the crests of the mountains are for the most part rounded and not jagged.

In Switzerland, therefore, there exists a limit at about 9000 feet in the centre of the Alps above which the summits are no longer polished, but where the rugged peaks present a very striking contrast to the lower surfaces, which are polished, or at least *moutonnées*. In the exterior chain of the Alps the polishing does not reach to a greater height than 6000 or 7000 feet. It cannot be doubted that this limit, which is so well marked, indicates the level of the bed of ice at the epoch of its greatest thickness.[5] This limitation of the glacial phenomena in the Alpine regions shows that an ice-sheet there is out of the question. This is shown again by the fact that the glacial phenomena culminate in the west, and gradually diminish in importance as we travel eastwards ; whereas an ice-sheet would distribute its products in all directions equably. It is again

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[4] Op. cit. pp. 413, 414.   [5] Ed. New Phil. Journ. 1842, xxxiii. 233.

shown by the limitations of area within which certain types of rock occur.

Escher von der Linth was the first, I believe, to definitely study the provénence of the various erratic blocks of Switzerland, and to show that the blocks in each valley had a common origin, but M. Guyot, by his elaborate examination of the question, reduced the phenomena to a law which is quoted under his name. He showed how in each valley the old erratics can be traced to their parent beds by separate and unmixed streams, showing that they had been distributed by local glaciers occupying the separate valleys, and not by a general overwhelming ice-sheet. In his memoir entitled *Ueber die Eiszeit*, Kinkelin gives a map of Switzerland, in which he has separately coloured the districts over which these various glaciers distributed their *débris* in the great glacier age, namely the shedding ground of the Rhone, Aar, Reuss, Linth, Rhine, Sentis, and the Black Forest glaciers. No more striking proof could be given of the fact that it is to very large glaciers with their movements conditioned by the contour of the country, and not to ice-sheets moving irrespective of such contour, that we must appeal to explain the phenomena of Switzerland. This is the view, so far as I know, of all the experienced explorers, both German and French and Swiss.

No doubt glaciers will not explain the whole of the Alpine drift phenomena. We cannot explain by them the far reaching beds of gravel and detritus spread in vast sheets over the low country surrounding the Alps, especially in its western parts. These are quite different in character to the moraines and other *débris* of ordinary glaciers, and we must invoke some other cause to explain them than local glaciers, but this cause was clearly something different in its nature to ice which is incapable of the postulated effects.

To this cause we shall presently revert. In regard to the phenomena of the Alps, I quite agree with Professor Judd, who says: "I know of no glacial phenomena which are not capable of being explained, like those of New Zealand, by a great extension of the area of the tracts above the snow line, which would collect more ample supplies for the glaciers protruded into the surrounding plains. . . . This increase of the area above the snow line, resulting in the extension of the

glaciers, would likewise follow from the elevations of the whole mountain mass ; and the same movement would also account for the transport of blocks across the wide Swiss valleys, and their lodgment on what are now opposing slopes. It is an interesting confirmation of these views that the Western Alps, where the subterranean action has been most violent and prolonged, is also the district in which the glacial action has been most powerfully active." [6]

Turning from the Alps to the smaller continental ranges, M. Falsan has pointed out how the same law applies to the Jura. Thus he says : "Depuis qu'en 1847 M.M. Lory et Pidancet ont signalé des dépôts glaciaires dans le Jura, aux Rousses, à Saint-Laurent, au Fort-de-Joux, à Sainte Croix, les faits observés ont prouvé que dans la plupart des vallées jurassiennes, des glaciers locaux ont existé d'une manière independante, pendant l'extension des glaciers Alpins."

He goes on to say that the ancient glaciers of the Jura extended from the Rhone to the Rhine, and towards the west they extended as far as Nantua, Oyonnax, Dortan, Champagnole, Nogeroy, Bonnevaux, Pontarlier, Locle, Noirmont, Porrentruy, etc. Beyond these limits there are also traces of isolated glaciers.[7] Turning to the Vosges the same author points out how its higher summits formed centres of dispersion for ancient glaciers which once filled the valleys and the cirques, and which radiated in various directions. Of this he gives ample evidence.[8] He further extends the same conclusion to the smaller mountain clusters of France, such as the Morvan, the mountains of Auvergne, the Cantal, etc., of which he says : "D'ou s'étaient échappés en rayonnant de toutes parts, jusque dans les plaines voisines, d'anciens glaciers." [9]

In regard to the Pyrenees, where the same writer describes the ancient glacial phenomena in detail, he concludes thus : "Au pied des Pyrenées les glaciers quaternaires, au lieu de s'epancher en immense mers de glace et de former en mêmes temps d'autres groupes isolés de moindres dimensions. . . . constituaient, sur une longueur equivalente environ à celle du glacier du Rhone, une série de 13 glaciers principaux, se sub-

[6] Judd, Geol. Mag. 1876, p. 537.　　[7] La Période Glaciare, pp. 308, 309.
[8] Id. 312-317.　　　　　　　　　　　[9] Id. 329.

divisant tous en glaciers secondaires." These glaciers he thus
enumerates: on the French side, those of the Ariége, of the
Salat, of the Garonne, of the Pique, the Neste, the Adour, the
Gave de Pau or d'Argelés, the Gave d'Ossau, the Aspe, and
the Saizon; on the Mediterranean, or south side, the glaciers
of the Aude, of the Tet and of the Tech.[1]

What is true of France is true also of Germany, where
the mountains of the Black Forest, of Saxony, etc., present
evidences of local glaciers having radiated from local centres
and distributed local *débris*, and not of ice-sheets.

Let us now turn to the British Isles, where it has been the
fashion, even among those who are not wedded to far-reaching
Scandinavian ice, to insist on the postulate of a great domestic
ice-sheet overriding the whole surface of these islands, except
perhaps the district south of the Thames.

Here, as in the north of Europe, we have a very serious
preliminary difficulty in reconciling two contradictions. If
boulders from the Scotch and Cumberland mountains made
their way to the Island of Lewis on the one hand, and central
England on the other, the rocks from which they were
detached must have reared their heads above the ice. If
they did so, how can we possibly secure such a surface slope
in the ice as would impel it, and with it the glacial beds, to
these far distances? We cannot secure an adequate slope.
Of this Mr. Geikie was partially aware.

He says of the Scotch ice-sheet:—" Measuring from the
Cliseam in North Harris to the mountains of Torridon, we
have a distance of fifty-six miles, so that the inclination of the
surface of the *river de glace* was very little, the fall not being
more than 1400 feet, or about one in 211. But slight as that
incline was, it was probably twice as great as the slope of the
river de glace that filled up the German Ocean.[2]

To this the Duke of Argyll replied that he doubted if there
was any evidence that ice could move to the extent required
on a slope of 1 in 211 or of 1 in 422. In such a case he
regarded it as impossible that gravitation could produce
motion.[3]

In this I completely agree. Assuredly the champions of

[1] La Période Glaciare, pp. 346, 347.
[2] Id. 861.　　　　　　　　　　[3] Q. J. G. S. xxxiv. 868-9.

ice-sheets should first prove the capacity of ice to move under such slight impulse as is here pointed at. Let us pass on, however, to the more local phenomenon.

First in regard to Scotland.

In a paper on the Glacial Phenomena of Scotland, by Mr. Dugald Bell, he is very strong on the point that the glaciation of Scotland was a local glaciation only. Answering Mr. Milne Home, whose view was that the boulders and striæ point to an external force from the north-west, he says that the boulders, by which the view is supported, occur on the *eastern side* of the principal mountain chains, *from which* it can be shown that the boulders and striæ radiated in all directions—north-north-west, west-south-west, and south as well as east or south-east. "Whenever we come to the *western* side of these mountains, the marks of glaciation are in a different direction, indicating a movement on that side *westward* out to sea, on this point we think a single day's excursion by Loch Earn or Loch Tay, up the valley of the Dochart to the neighbourhood of the Black Mount, and then down Glen Etive, or Glencoe, on the other side, is sufficient to convince anyone who fairly observes the facts. Similar facts are presented very clearly in the south of Scotland. The high ground of Carrick and Kirkcudbright has evidently formed a centre of dispersion, from which the striations and boulders can be traced, extending outwards on every side—to the north and west, as well as to the south and east."

Mr. Bell says, "In the pass or valley between Arden and Helensburgh, the glaciation is from the north-west, a little down the firth it is as clearly north and north-east, and in parts of Ayrshire from the south and south-east, dependent entirely on the position of the locality in regard to the principal mountain ranges."

Mr. Home is himself constrained to admit that all the boulders and rock striations at the Gareloch, Loch Gair, and Loch Ayre, point to a north-north-east direction of transport, and that in this district there may have been a current of floating ice moving in a south-south-west direction.

In regard to Loch Etive, we are told that the rocky knolls along its course are all smoothed from the east, "indicating apparently the action of a glacier which had moved down the

valley now occupied by the loch." [4]   Mr. Bell says " that the facts collected regarding the transport of boulders at the northern or Inverness end of the Crinan Canal, leave no doubt that the general movement in that quarter was from south-west to north-east, up the line of the Great Glen, and out to sea by Forres and Elgin ; . . . in the southern half of the Great Glen the direction of boulder transport has been clearly to the south and south-west." This change in the direction of transport along the line of the Great Glen to the north-east at one end of it, and to the south-west at the other, is assuredly a very important piece of evidence in support of the local character of the glaciation, and against the notion of ice-sheets moving athwart the whole country. [5]

In reference to the argument derived from boulders perched on the ridges and tops of " the highest hills," which it has been urged are due to the westerly movement of the great *mer de glace*, which was powerful enough to override the watershed, Mr. Home says, " I confess that I have never been able to understand how boulders carried either on the surface of a glacier, or a *mer de glace*, could be left perched on the ridges, and still less on the tops of the highest hills." [6]

Jamieson remarks how on the east coast of Aberdeenshire the striæ run from west to east, at Elton nearly due east and west. At Aberdeen itself they run a few degrees south of west, in the direction of the valley of the Dee, the rounded and polished faces of the rocks look up the valley. On the south shore of the Moray Forth, between Banff and Troup Head, they point south-east and sometimes south-west, in the Firth of Forth from west to east, and at Stirling from north-west. On the west coast the case is very different. There it is the east and north-east points that are worn down. The striæ that streak the rocky shores of the fiords of Argyle-shire are just such as might be expected from the action of ice moving down from the mountains ; the rounded striated surfaces are always presented to the interior. Professor Nicol has also chronicled the direction of the striæ as noticed by Sir Roderick Murchison and himself, in several of the

[4] 7th Report on Boulders, vol. xi.
[5] Trans. Geol. Soc. of Glasgow, viii. 227-247.   [6] Id. 360.

glens along the east, north and west sea-boards of Ross and Sutherland, all pointing to the same conclusion, viz. that they are due to the passage of ice down the glens.[7]

The same writer says elsewhere of the striæ in Scotland : " In the open country, and on the tops of ridges, the direction of the furrows is generally very uniform over wide districts ; but in the deep mountain valleys it conforms, as a rule, to the direction of the glens. Taking the country as a whole we find, on coming to map the markings, that they radiate from the chief mountain masses of the interior, and that the rubbed faces of the rocks look towards the great watersheds."

Mr. A Somervail says emphatically, that the area of Scotland is not of sufficient extent to admit of the accumulation of ice upon it to a depth of 3000 feet. " Perpetual movement," he says, " from a higher to a lower level by virtue of the law of gravitation or regelation, or a combination of both, would, in an area so small, prevent the ice forming to such a thickness, independently of the hot summer of perihelion . . . . In regard to striæ occurring at a height of 3500 feet, and thus requiring a thick sheet to explain them, he very properly calls attention to the fact that the striæ in many cases correspond to the slope of the ground. " We possess," he says, " substantial evidence from the trend of the striæ, that the ice conformed to, and flowed in a line with, the great valleys, nay, that as a general rule the striations, wherever they occur, are in harmony with the present drainage of the country ; that the ice from the Grampians, as asserted by both Messrs. Croll and Geikie, should have filled up the great valley of Strathearn, and flowed over the tops of the Ochils and Pentlands, is a baseless hypothesis. The existence of till on the top of both these ranges is incorrect. On the summit a peaty soil certainly occurs, formed partly by the decomposition of vegetable and mineral matter, in which are included numerous fragments of rock, both of local and Highland origin ; but to term this accumulation till or boulder clay is an abuse of language, and to infer from the loose and imbedded pieces of mica schist that the Highland ice overflowed these hills, is a most unwarrantable conclusion. The

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[7] Jamieson, Q. J. G. S. xviii. 167, 168.

striations mentioned by Dr. Croll as occurring on the top of Allermuir, very unfortunately for him and Mr. Geikie, trend in the wrong direction, and are opposed to the idea of an ice-sheet coming from the Grampians sweeping over the Pentland range. Striæ," he adds, "observed by Mr. John Henderson, ran north-east by east, and south-west by west." [s]

In a subsequent paper on the Higher Summits of the Pentland Hills, containing a careful description of the loose materials that occur there, including the foreign boulders and stones, the same author says he entirely dissents from the position that they were brought there by an ice-sheet from the Highlands, and considers the position as quite untenable, and as failing to explain the phenomena. He also contests the facts alleged by Croll as to the occurrence of boulder clay on the top of Allermuir Hill, and says, " I hold that neither on Allermuir, nor yet on the top of any other hill in the whole range, is there such a thing as typical boulder clay, but only a peaty soil partly formed by the underlying rock and the decomposition of the boulders themselves, together with the decay of vegetable matter, making up a clayey deposit, but having no connection whatever with boulder clay. It is clear that no glacier of Highland origin can account for the *débris* of the Cairn Hill sandstones resting on the other hills of the range. Instead of ice moving from the Grampians, there is rather the proof of some force from an opposite direction, viz. from south-west to north-east, or in a line with the exact trend of the hills, or very near it. There are other facts pointing the same way. In deep excavations made in the boulder clay at Alnaich Hill, near Liberton, there were exposed many boulders of old red sandstone, which must have come from the vicinity of Carlops. I also saw many large boulders of the same rock, along with others from the Pentland range, dug from cuttings made in the boulder clay two years ago at Seafield, near Leith. So that all the boulders observed must have travelled in a line from south-west to north-east, or parallel with the range of the Pentlands, from whence they were derived. Mr. John Henderson has likewise noted the fact of a large slab of sandstone lying in the

gorge of the Bonally Burn, which belongs to beds about half a mile to the south-west . . . . I think we have at least two well-defined movements from different directions: the one from south-west to north-east, the other from about north-west to south-east. The former movement was that which carried, in a line parallel to the trend of the chain, the fragments of the cairns over the tops of the other hills, and swept masses of the old red conglomerate and porphyrites in the direction of Seafield. To the latter movement from the north or north-west, crossing the hills at right angles or diagonally, I would refer the large boulders of quartz and mica slate resting on the slopes of the Pentlands, and ' the White Stone of Peebles' noted by Mr. Ralph Richardson."[9]

Speaking of the Urr Valley in Kirkcudbrightshire, the same geologist remarks: " The trend of the striæ, as well as the boulders, all point to the conclusion that the glaciers conformed to the present system of valleys in this region as in others."[1]

After the reading of Mr. J. Geikie's paper on the Glaciation of the Hebrides, which he attributed to ice coming from the south-east, a telegram was received from Dr. Bryce, in which he said he had satisfied himself from observations made in Lewis, Harris and North Uist, and had published his opinions that the ice which had produced the glacial markings in those islands had come from the west. Mr. J. E. Campbell, in a paper read after Mr. Geikie's, and containing many observations, came to the same conclusion.[2]

At the Glasgow meeting of the British Association, the Duke of Argyll, Professor Harkness, and Mr. Jolly, of Inverness, also gave decided testimony in opposition to Mr. Geikie's views.[3]

The Duke points out that the granite blocks at Iona came from the Ross of Mull. They, no doubt, were derived from the glaciers on Ben More. " The movement of an ice-sheet from the north-east or the north could not have carried a single fragment of granite to Iona." Mr. Allport, in a paper on the

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[9] Trans. Geol. Ed. Geol. Soc. iii. 197, 198.   [1] Id. 250.
[2] Q. J. G. S. xxix. 545, etc.
[3] Trans. Geol. Soc. of Edinburgh, iii. 102, 103.

igneous rocks of Arran, says he did not find a single boulder which did not belong to the island itself.[4]

In answer to Mr. Geikie's arguments in the same paper, Mr. Belt said that the evidence was unsatisfactory, and some of it pointed to the opposite direction. The fact that more till was left on the west side of the islands than on the east, was inconsistent with the idea that the ice came from the latter direction. It was during its retreat that the ice would deposit mud and stones on slopes opposite to its course, as then the water issuing from the retreating glacier would be ponded back, while on the slopes coinciding with its flow the water would find a free outlet, and carry away the mud, as it does now in the Swiss glaciers.

" The hills of Skye lay directly in the course of the supposed flow of ice from the mainland, and their rocks should have been largely represented in the boulders of the Outer Hebrides, on Mr. Geikie's theory, but they are entirely absent. Even if the ice at one time passed completely over the Outer Hebrides, a time would come in its retrocession when it ought to have left its terminal moraine there, and in this the rocks of Skye should have abounded. Their absence seems to be fatal to the theory." The Duke of Argyll says, " The absence of fragments from the mainland and Inner Hebrides upon the Outer Hebrides seem to show that no great body of ice could have passed from the former to the latter." Professor Judd says that, "while boulders derived from Skye, Burn, Mull, etc., did not go to the Outer Hebrides, and on Skye itself rocks from the mainland are very rare, yet boulders derived from the Inner Hebrides are certainly found on the west coast of Scotland."[5]

This will suffice in regard to Scotland. No doubt there, as in the North of Europe, we have to face difficulties which local glaciers will not explain, and for whose explanation the theory of "the major glaciation" has been invoked, a theory which, as we have seen, is inconsistent with the physics of ice. To these difficulties we shall revert in the next chapter. Meanwhile, it seems clearly established that the actual handiwork of ice in former times in Scotland can be reduced as elsewhere

[4] Geol. Mag. ix. 537.   [5] Q. J. G. S. xxxiv. 867-869.

to the action of local glaciers, moving as modern glaciers move and doing the same work.

Let us now turn to England, and begin with the north country, and with a very considerable admission made by Professor Geikie himself.

He speaks of a debatable land between the northern and southern currents of ice. " The right of possession to the hilly tract," he says, " that lies between Paisley and Kilmarnock, seems frequently to have been disputed by the rival ice streams, the rocks of that area being sometimes striated from north to south, and sometimes in the opposite (? transverse) direction. We find, also, an inter-mingling of stones, fragments of mica schist and gneiss from the Highland mountains occurring now and again in the till of the valley of the Irvine ; while stones derived from the high grounds to the south of that river, appear here and there in the till that sweeps up to the crests of the hills overlooking the basin of the Clyde. A similar intermingling of stones from the north and south is seen in the till of the valley of the Esk, near the Moorfoot Hills in Edinburghshire." [6]

The boulders from Galloway have travelled eastwards towards Newcastle, south-east up the Eden valley and over Stainmoor to the vale of York, and south as far as Ravenglass, thence to Manchester. Professor Sedgwick has mentioned the occurrence of boulders from the Lake district, in those parts of Galloway which have furnished the largest number of boulders to the Lake district itself. Even in those parts of the lake district in which the majority of the boulders have moved outwards at low levels, we find that some of the very same rock have been transported in an opposite direction towards the heart of the mountains. The peculiar granite of Dufton, close under Cross Fell, has sent blocks among other directions to Shap, " where," says Mr. Goodchild, " I have picked them up out of the till," while, on the other hand, the well-known granite of Shap is well represented by several large blocks close to the Dufton granite *in situ.* The currents that took them have been crossed at right angles by others which took the red syenite of Bullermen and the numerous

[6] The Great Ice Age, 91, 92.

kinds of granite and other crystalline rocks from Galloway, up the Eden valley and over Stainmoor.[7]

In a paper on the Wastdale Crag Blocks, Professor Harkness says they never occur in the boulder clay, but on it, while the Criffel granite from Criffel, near Kirkcudbright, does occur in the clays. " Glacial striæ," he says, " may be seen in several localities in Westmoreland, and *wherever they occur their direction corresponds to the course of the valleys.* This is the case, not only with the vales of the Lyrennet and the Eden, but also with those small dells which contain the tributaries of these streams. The same circumstance is seen likewise in the valley of the Lowther, a river which has its origin in part from Wastdale Crag, and which, after flowing northwards, joins the River Eamont about a mile south of Penrith. These striæ, formed by ancient glaciers, and running in the direction of the present valleys, indicate that the drainage of the country and its present outline have not been much altered since the operation of glacial action.[8] . . . The ancient glaciers of the vales of Lyrennet and the Eden have followed the courses of the present drainage of the country, or, in other words, have had a motion from south to north. . . . Independently of the direction of these old valleys, the outline of the country with its slopes between Wastdale Crag and that portion of the Pennine chain which has been crossed by the granite blocks, is altogether hostile to the idea of a glacier (rather an ice-sheet) having been the transporting agent of these masses of granite. The prominent escarpments fronting this glacier would have diverted it from an eastern course, otherwise these escarpments would have been planed down by its action, and all traces of the north and south striæ, which now mark the faces of the rocks, would have been obliterated, and their places occupied by others of more recent date, running from west to east, yet no such markings are to be found. The idea, therefore, which suggests a glacier as the agent of the transport of blocks has very little to support it." [9]

Mr. D. Macintosh says, that " In the Eden valley the dispersion of boulders of Criffel granite is so interwoven with that of boulders of porphyry and syenite from the mountains of

[7] Goodchild, Geol. Mag. xi. 504, 505.
[8] Q. J. G. S. xxvi. 523.   [9] Id. 524, 525.

Cumberland, as to be incompatible with theories of transportation by currents of land ice; that the same interweaving of Criffel and Shap boulders in the lower Eden valley is inexplicable on a theory of upper and lower ice currents; and that the limitation of Criffel boulders about the north and west borders of the Lake district mountains, to a few hundred feet above the present sea-level, is inconsistent with a boulder charged ice current 2400 feet thick."[1]

Mr. Ward showed very clearly that the ice working in the Lake district was local ice. He says: " If the glaciating agent had come from the north we should expect to find boulders of Skiddaw slate, Skiddaw granite and the metamorphic rocks associated with it upon the area occupied by the volcanic series; if from the south to north generally, we ought to find boulders of the volcanic series upon the Skiddaw slate area. The fact is, over the district under consideration not one boulder of Skiddaw slate, Skiddaw granite, or the associated metamorphic rocks, has been found upon the area occupied by the volcanic series; while boulders of the volcanic rocks occur in thousands over a large part of the Skiddaw slate area up to certain heights. Moreover, among the transported blocks there are not found any of rocks foreign to the district as a whole. If it be urged that the effects of the northern ice-sheet were effaced by subsequent local glaciers, surely some foreign boulders should have been left; at any rate, the burden of proof lies with the advocates of this great mountain ignoring ice-cap. . . . The direction of the ice-scratches, the way they run along with the main valleys, although sometimes systematically crossing low water-sheds, at once suggests sheet or glacier ice originating in the district. . . . The moraines, for the most part, bear witness to glaciers, each confined to its own valley . . . it is evident that the direction in which the boulders have travelled agrees with that of the uniform ice scratches."[2]

The striæ on the line of fells ranging through High Street to the east side of the foot of Ullswater, and the higher range from Helvellyn towards the high ground about Caldbeck fells, show that the ice moved in the main along the valleys.

[1] Q. J. G. S. xxxi. 692.   [2] Id. xxix. 422, etc.

Again, in regard to the boulders which have been carried over higher ground than their original site, such as the Silurian erratics of Wharfedale, Mr. Dakyns says: "They occur solely in the drift of the plain, and would seem to have come on floating ice, for it is pretty clear that they were not pushed by an ice-sheet over the fells, as they are not to be found anywhere on the high ground; and it is, in my opinion, quite impossible that the Ribblesdale glacier should, after debouching on the low ground at Settle, have spread out at right angles to its course for the distance of twelve miles."[3]

As Mr. Eccleston has found blocks of Eskdale fell granite more than 1000 feet above the sea on the west side of Blackcombe, and this stream must have carried the granite as far south at least as Bridgnorth and Wolverhampton, on the Pennine hills and in the Yorkshire valleys, south of the latitude of Wastdale Crag, the drift carriage has either approximately followed the valleys, or come chiefly from the north-west or north-north-west—(on the north side of Remballs moor, however, Millstone Grit has been beautifully smoothed and striated west and east at a height of about 900 feet above the sea)—if we except chalk flints which may have come from Ireland. Fragments of Wastdale Crag granite are found in a brown boulder clay west of Ripon, and, according to Professor Green, a boulder of this granite may be seen at Royston near Barnsley.

Though the granite in the Wastdale area rises to about 1600 feet above the sea, Wastdale Crag itself, or rather Wastdale Hill, whence most of the boulders come, is 1479 feet above the sea level. An ice stream low enough to receive boulders on its back would not have been more than 100 feet thick on Stainmoor pass. Could such a thin stream have traversed the pass and pushed the blocks on, how did the eastern stream acquire impetus, not only to enable it to ignore minor ridges, but to traverse hills in east Yorkshire more than 1000 feet in height?

Again, Wastdale Crag granite has found its way as far as the mouth of the Humber. How has it expanded from a narrow reach on Stainmoor, over an area of 40 miles in breadth from

[3] Geol. Mag. 1st Ser. x. 63.

Ripon and York to the coast, while liassic drift over which the granite must have passed, has been dispersed over the plain of York. Gryphæa incurva has been found in boulder clay as far west as Keighley. Blocks of Permian breccia have been moved up a slope to a vertical extent of 1000 feet.

" Again, we must take into account the radiating character of much of the dispersion. That land ice could have dispersed boulders in various and often opposite directions from detached or semi-detached hills of moderate elevation appears to me almost impossible." [4]

Mr. Dakyns says, "The North and West Ridings of Yorkshire were like other mountain groups, to wit, Norway, Scotland, the Lake Mountains, and Iar-Connaught glaciated radially; that is, the ice flowed outward from the central fells in all directions, much in the same way as the rivers do now; the snow and ice drainage was broadly analogous to the water drainage. The great dales, Wensleydale, Wharfedale, Nitherdale, Ribblesdale, Dent, contain no foreigners in the drift. The drift material, is, as far as I know, entirely composed of rocks from their basins."

Mr. T. M. Reade, in an elaborate and detailed survey of the drift deposits of Lancashire, Cheshire, and North Wales, after discussing the provénance of many of the boulders, says : " If these identifications are correct, it will be seen that all the stones are confined to the basin of the Irish Sea, and the river basins flowing into it, excepting some stray stones that may have come from the Highlands of Scotland. . . . No Shap fell granite has ever been found by me ; nor have I ever heard of its having been found on the west side of the Pennine chain— another fact proving that the erratic rocks of the area under consideration are confined to the drainage basin of the Irish Sea. This fact seems to me fatal to the idea of an ice-sheet overriding the great water-sheds, and points to a system of glaciers radiating from mountain nuclei." [5]

Again he says : " If there ever existed this extension of ice from the mountain centres (for I have observed no facts to warrant in the slightest degree the idea of a Scandinavian ice-sheet extending over our island), it must have melted back

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[4] Mackintosh Geog. Mag. viii. 94, 95.   [5] Q. J. G. S. Mag. 1883, p. 120.

and separated into local glaciers, *before the drift deposits I have described were laid down.* During the period of the great submergence, it is as clear to my mind as anything can be in so difficult a geological subject, that no ice-sheet existed in any part of Lancashire that I have examined." [6]

Mr. Aitken, who knew the Lancashire drift beds very well, says: "I do not think there is any trace of a large sheet of ice having moved bodily over the whole district and not conformed to the direction of the valleys in this neighbourhood. Further north the statement may be correct, but I do not think such a thing occurred in Lancashire."

"Observations made in the valley of the Mersey," says Dr. Ricketts, who is a champion of much more advanced glacial views than I hold, "are altogether at variance with the idea of such an enormous ice-sheet as has been supposed. There are abundant evidences that it and its tributaries were filled with glaciers not progressing from an immense accumulation in the north, but formed by the snowfall in the respective valleys, being of such an extent only as might reasonably be considered due to the amount of deposition on their water slopes. . . . The direction of the striæ varies in different places, but, so far as I have observed, always take a direction similar, or nearly so, to that of the valleys in which they are situated. . . . In the section exposed in the making of the Huyton and St. Helen's Railway, and near Whiston, accumulations of unstratified materials, derived from the adjacent rocks, consist of loose sand where there are triassic rocks, and of grey mud containing fragments of coal where there are coal measures. Combining these collections with the striated rock surfaces, they can only be considered as moraine *débris* . . . every phenomena is in direct antagonism to the hypothesis of a monstrous glacier filling the Irish Sea, or of one extending from the mountains of Cumberland across the Bay of Liverpool, and over the Island of Anglesea. With such supposed barriers, no sea could have existed between them and the mainland; none of those pebbles with which the boulder clay is studded could have passed over and reached the situations where they are embedded in it; least of all could those ancient

[6] Q. J. G. S. Mag. 1883, pp. 121, 122.

Irish immigrants, the flints and pieces of chalk from the Antrim district, have come with such an obstruction." [7]

In North Wales the great mountain bosses of Snowdonia were no doubt the home of their own local glaciers, a fact which I do not know that anyone disputes. These glaciers were the distributers of *débris* in various directions, and notably in the English Midlands. On the northern borders of the hill country, the local drift is mixed with that which has travelled from the north, but over the district of Snowdonia the phenomena are universally admitted to be explainable only by local glaciers. Professor Hughes has urged that the true glacial beds here contain only local stones, the foreigners came when the rearranged or marine drift was deposited.

In regard to Central Wales, Mr. Keeping says : " That it is only at the north and south borders of Cardiganshire that any erratic blocks occur whose origin cannot be referred to the present valley systems. In the valley of Llyfnant large blocks of felstone occur, which can only have come from the mountains further north in the Cader Idris or Aran ranges. . . . This is the only case I have met of the invasion of Central Wales by the erratics. Boulders from Carmarthenshire occur in the south of Cardigan. . . . Excepting the extreme north and south, the glaciers were of strictly local origin, each confined to its own valley, there being no evidence of any confluence of the ice into a great mer de glace." [8]

In South Wales the evidence is the same, and Mr. Horace B. Woodward points out that with the exception of some chalk flints which are met with in places, the boulders in the drifts of Brecknockshire and Glamorganshire are almost entirely of local derivation. [9]

In a paper by Mr. J. W. E. David, on Glacial Action in South Brecknockshire and East Glamorganshire, he tells us all the erratics are of local origin, with the exception, perhaps, of the chalk flints of the Ely valley, and possibly of a small block of quartz porphyry found in an old wall at Duffryn, Golwg, St. Nicholas, Cardiff. [1] In the discussion on it,

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[7] Proceedings Liv. Geol. Soc. 1876, 1877.
[8] Geol. Mag. 1882, 251-256.
[9] Geology of England and Wales, 493.   [1] Q. J. G. S., 39-41.

Professor Prestwich remarked that one distinction in the district was that the erratics were local.

If we leave the mountain country and travel into the lowlands of central and eastern England, the same conclusion seems inevitable as in the case of North Germany, namely, that we must invoke some other element than an ice-sheet if we are to explain the facts.

Mr. Crosskey, in describing the erratics on Frankley Hill, says: "None of the well-known Scotch and Lake types so abundantly scattered over many parts of the Midlands, occur there. After visiting the blocks *in situ*, Mr. Bonney pronounced them of Welsh origin and of lower Silurian age. Similar Welsh felsites occur on Romsley Hill, separated from Frankley Hill by a narrow valley, and at a height of 897 feet. In the former case the felsite boulders are mixed with jurassic and other boulders from the locality itself, all being striated. This points to this locality in glacial times not having been overridden by a great ice-sheet from the north and north-east, but from Wales." [2]

I may here remark that in the neighbourhood of Wolverhampton and Stafford, as Mr. Mackintosh has pointed out, there seems to be a meeting ground of erratics from the north-north-west, and others from the east-north-east, the former chiefly granite and felstone and the latter chiefly cretaceous and jurassic. This double provénance of the drifted stones like the similar puzzle in the glacial beds of Yorkshire and Eastern England, where some have come apparently from Norway, others from Scotland and others again from Cumberland, points to radiating centres of dispersion and is unmistakably opposed to ice-sheets moving continuously in one direction and overriding obstacles. I may say that the same conclusion seems to me to follow from the occurrence of great masses of chalk isolated and a long way from any chalk *in situ*, such as those at Sywell in Northamptonshire, at Redlington in Rutlandshire, and at Stukely in Huntingdonshire. [3]

Mr. De Rance tells us how he found in the glacial sands of

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[2] See Glacial Geology of the Midlands, Proceedings Birm. Phil. Soc. vi. p. 169, etc.　　　[2] Horace Woodward, op. cit. 496.

Leamington, in Warwickshire, corals from the lias of Chelten-
ham, pectens and other fossils from the red crag and a nautilus
from the London clay,[4] while the boulders in the clay came
from the north.

Mr. Jukes Brown, writing of the drift deposits of Lincoln-
shire, says: " The boulder clay is not disposed in the manner
of moraines, but was clearly spread out as a universal mantle
over the whole surface of the country.   The ice which produced
it certainly could not have been generated on the ridge itself,
nor on any of the neighbouring hill ridges, and yet the
materials of which the clay consists, and nearly all the stones
it contains, are essentially local products derived from the rocks
in the immediate neighbourhood.   It is obvious that the chalk
fragments must have been brought from the N.E.   The
carboniferous rocks can only have come from the N. or N.W.,
and the marlstone blocks travelled in all probability from
W. or S.W. of the places where they are found. . . . When
we consider the remarkable distribution of the stones and
boulders in the clay of this area, the greater proportion of
chalk detritus on the eastern slopes and of jurassic detritus
on the western slopes, the fact that enormous masses of marl-
stone occur many miles to the eastward of the only places
whence they can have been derived ; the position of the large
boulder of cornbrash, near Ingoldsby, and the occurrence of
lower lias limestone at Croxton 300 feet above its level; the
steep slope of the oolitic escarpment, up which the ice must
have passed, the difficulties in the way of applying the preva-
lent land ice hypothesis become considerable.[5]   Near Broms-
grove triassic pebbles have been found mixed with Scotch,
Cambrian and Welsh erratics.[6]

Mr. S. Wood, jun., argues against Mr. Croll that the for-
mation of the Yorkshire boulder clay, by means of an ice-
sheet, is contradicted by the abundant beds of sand and gravel
intercalated in it, in one of which occur shells, perfect, un-
rolled, and sometimes double, and that his explanation of the
absence of chalk from the purple clay by one arm of the ice-
sheet having passed north of and so escaped, the Wold is equally

      [4] Geol. Mag. viii. 415.
      [5] Memoir on S.W. Lincolnshire, Sheet 70, 82, 3..
      [6] Q. J. G. S. xxxvi. 182.

negatived by the fact of this clay overlying and gradually passing down through clay with more and more chalk into the chalky clay along the Holderness coast, viz. at Dimlington cliff and at the cliff south of Mappleton. Besides the gradual transition, these places could not by any possibility be reached from the direction of Shap without the chalk being crossed.[7]

The same writer further urges that the chalky boulder clay of the Eastern counties overlies purely stratified sand of indisputable marine origin, the junction between the two being quite undisturbed, presenting an even line of transition, while occasionally a passage bed of interstratified clay and sand occurs. If the land ice had passed over and deposited the clay or squeezed it down, the sands must have been squeezed up and contorted, instead of the stratification being undisturbed right up to the point of junction. Sometimes the clay dips into the sands and the sands are found undisturbed, except in the place ploughed ; occasionally threads of sand are found in the unstratified clay, and if there are two their horizontality is preserved. Sometimes masses of clay are found imbedded in the sands, whose stratification above and below it remain intact.[8]

Mr. Seeley says of the Fenlands : " I have not found in this locality a vestige of iceberg action. Of glacier action the deposits of the Fenlands offer no trace, unless the fragments of northern rocks be held to prove that one great glacier stretched from the Tweed to the Thames, of which there may be as much likelihood as that the ice of the Caucasus excavated the Black Sea." [1]

If we put aside England and go westwards, we have to meet those who have argued that the Irish Sea, like the North Sea, was once filled with a great sheet of ice which distributed the northern *débris* as we find it, and which, like the North Sea ice, is supposed to have pushed the shells of the high level marine drifts out of the ocean beds on to the hill sides, many hundreds of feet above the sea level. This ice-sheet is supposed further to have overwhelmed both the Isle of Man and Anglesea. In regard to the shells, I need not repeat the ar-

[7] Geol. Mag. viii. 92.   [8] Id. vii. 347.   [1] Id. iii. 496.

guments I have already used about the North Sea, all of
which apply here. The shells do not attest strictly arctic
conditions, nor could they have lived in a sea choked with
ice, nor could this ice have mixed shells from different depths,
and mixed them with long travelled pebbles and boulders
without entirely crushing them. Nor could it, as we have
seen, have taken them up and transported them uphill for
many hundreds of feet.

Again, while pebbles have been brought to North Wales
from Galloway and Cumberland, chalk flints have been carried
right across this line of portage from Ireland. In order to
get over this and similar difficulties the advocates of ice-sheets
have had to invoke some fantastic arguments. Thus, Mr.
Horne believes, from the presence of chalk flints on the Isle
of Man, that the Irish ice at times succeeded, "after a hard
battle, in reaching the Manx coast. If so, it must have to a
certain extent displaced or 'shouldered aside' the Scotch
and Cumberland ice. It is not to be supposed that the normal
momentum of the respective ice-sheets remained constant.
The moving force must have varied with changing conditions,
and the respective ice-sheets must have been subjected to
intense compression." [2]

If we turn from the Irish Sea itself to the islands in it, the
evidence seems to compel only one conclusion.

Thus Mr. Ward says of the Isle of Man : " There is a great
absence of boulders on the mountain sides of that island, and of
large *foreign* boulders on the mountain slopes. Boulders of
local greenstone and granite do, however, occur in this way,
and sometimes in very anomalous positions, such as the granite
blocks on the west side of South Barrule, the granite itself
being on the east side at a much lower level."[3] He argues
that there are few signs of land glaciation, compared with
the results of marine submergence, and says : " The sandy
glacial deposits, especially developed in the north of the
island, have yielded many remains of shells, and are un-
doubtedly for the most part marine in origin. A few miles
south of Point of Ayre, these sandy beds are contorted
and even inverted. Moreover, the glacial drift runs up

[2] Sketch of the Geology of the Isle of Man, 23.
[3] Geol. Mag. Ser. II. viii. 7.

into the interior of the island in the form of gently sloping plateaux, being, as I believe, the indication of marine action upon the first formed glacial till." [4] He argues that the Isle of Man shows signs of having been very considerably submerged to an extent of 2000 feet. He thus accounts for the blocks at St. Barrule, which are inconsistent with an ice-sheet coming from the north, for the boulders have moved in a west-south-west direction, and have been lifted up 700 feet. Another fact pointing the same lesson is the absence of Alpine plants in the Isle of Man.[5]

Again, in the Isle of Man, while the boulders have travelled from the north-north-east and perhaps north-west, the striæ point to the north-east. This is assuredly very inconsistent with an ice-sheet having overwhelmed the island, as is so often urged.

Dr. Ricketts, in regard to Ramsay's notion of an ice-sheet moving down the Irish Sea and overwhelming Anglesea, says: "There are exposures in Anglesea which afford evidence that the latest glaciation of the island did not result from such a system of glaciers as has been supposed. Thus in the circumscribed area of Serpentine, lying a little south of Valley Station, on the Holyhead Railway, there is exposed a smooth and striated surface covered with a moraine *débris*, so completely derived from the immediate locality that it *entirely* consists of angular fragments of the Serpentine itself; and at Llanerchynudd, on the Anglesea Central Railway, the striæ on a base of rock, situated close to the station, have a direction towards south 10° west, varying as they curve round it to south 20° west." Professor Ramsay remarks that the glacial striæ of the Island of Anglesea run generally in a direction from 30° to 40° west of south, and that " these striations point directly to the mountains of Cumberland," but this is also very nearly the same direction as that of the pre-carboniferous valley, in which, on its subsidence, the carboniferous limestone and millstone grit were deposited; so that the glaciers flowed in channels which had been marked out by the comparative hardness of different rocks that have existed from palæozoic times." [6]

[4] Geol. Mag. New Ser. vii. 6.   [5] Id. 8.
[6] Proceedings Liv. Geol. Soc. 1876, 1877.

Let us now turn to Ireland.

Professor Hull, who is a champion of ice-sheets, is constrained to admit that in Ireland we have unmistakable evidence of local glaciers. Across the Island in the north, between Lough Corrib and Lough Mask in the west and Lough Neagh in the east, Mr. Maxwell Close has shown that "there exists a tract of country which has been the axis of motion for the ice in opposite directions seawards." [7] The same writer says: "The several mountain groups and ranges lying to the north-west, south and east of this region, were doubtless themselves centres of local accumulation of snow and centres of glacial dispersion. Amongst these mountains the vestiges of local systems of glaciers, in the form of masses of moraine matter, are often apparent." [8]

Elsewhere, Professor Hull, speaking of local glaciers in Ireland, says: "They have been noticed by Campbell and the officers of the Geological Survey, amidst the Donegal Highlands; by King, Close, and Kinahan among the mountains of Galway and Mayo; by Tyndal, Jukes, Close, and the officers of the survey among the Reeks of Kerry and the Dingle Highlands; by Du Noyer among the Commeragh Mountains of Waterford, and by Jukes, Close and others amongst the mountains of Wicklow, and of these I have already described examples in the case of the moraines of Glenmalure, Glendalough, and Glendaran." [9]

In a paper read before the British Association, Mr. Griffiths, while urging that the majority of the boulders in Ireland seem to have drifted from the north-west to the south-east, adds that this was not the case everywhere. This he concludes from the boulders of brownish-red sandstone covering the surface of the limestone valley between the Curlew and the Ox Mountains, the Ox Mountains containing eskers of limestone gravel, evidently derived from the limestone valley of the south and the granite boulders of Easky, and of the north coast of Mayo derived from the Ox Mountains, also situated to the south, whence we may arrive at the conclusion,

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[7] Hull Phys. Geol. and Geog. of Ireland, 1891, 257.
[8] Id. 265, 266.　　　　　　　　　　[9] Id. 297.

that at least in the localities mentioned the current has been from the south and not from the north-west.[1]

Mr. Campbell says: " All recent glacial marks that I could find indicate a local system of Donegal glaciers, which moved as ice now moves about Bergen and in Iceland. In particular, the ridge which divides Glen Veagh from Gweebarra Pass was covered by a sheet of ice like Lang Jökull in Iceland, which flowed off it as water flows off the roof of a house into gutters. That which is true of Donegal, is true of all the groups of mountains which I have visited in Ireland.

"A smaller local system left conspicuous marks on the peninsula which ends in Slieve Liag. Another was in the Antrim Mountains. These moraines are entirely made up of fragments of rocks of Antrim. . . . Another large local system was in the Mourne Mountains. Another had the Twelve Pins and other hills of Connemara for gathering ground and starting-point. Another was in the group of hills on the west side of Lough Neagh ; another was in the Sligo hills ; another was out near Achil Head. A very large system was in the south-west corner of Ireland, with the high grounds about Killarney for gathering ground." [2]

The evidence of Great Britain, therefore, is consistent and is emphatic, that while we have large traces, especially in the low country, of the operation of some force which has distributed the drift and rearranged the glacial beds, this could not have been land ice, and in regard to land ice action the evidence points here as elsewhere to the existence in the so-called glacial age of greatly enlarged glaciers, like existing glaciers in their mode of operation and in the work they did, only on a wider scale, and not to ice-sheets with the transcendental powers which have been attributed to them.

Let us now cross over to America. In America there is a very interesting but curious initial problem to solve, namely, to explain how the drift should have been transported so far and yet no farther. It is very curious that as we near the Rocky Mountains the great blanket of drifted sands and clays disappears, and further west we meet with nothing of the kind, and only with traces of former glaciers having existed on a

[1] Report Brit. Assoc. 1843, page 41.   [2] Q. J. G. S. xxix. 210.

much larger scale than those now existing, and no one, so far as I know, has ever postulated an ice-sheet as having existed in the western parts of America. This, as we shall see, has some important lessons for us.

Whitney says: " The explorations of the Geological Survey of California have demonstrated that there is no true northern drift within the limits of this State. . . . There is nothing anywhere in California which indicates a general glacial epoch, during which ice covered the whole country, and moved bodies of detritus over the surface independently of its present configuration as is seen throughout the North-Eastern States." [3]

Dall adds, " The same is eminently true as far as we know of Alaska." [4]

Clarence King, a most experienced observer, writes :—

" In the field of the United States Cordilleras, we have so far failed to find any evidence whatever of a southward-moving continental ice-mass. As far north as the Upper Columbia, and southward to the Mexican boundary, there is neither any boulder clay nor scorings indicative of a southward-moving ice-mass. On the contrary, the great areas of Quaternary material are evidently subaerial, not subglacial. The rocks outside the limit of local mountain glaciers show no traces either of the rounding, scoring, or polishing which are so conspicuously preserved in the regions overridden by the northern glacier. Everything confirms the generalization of Whitney as to the absence of general glaciation. . . . Wherever in the fortieth parallel area a considerable mountain mass reached a high altitude, especially when placed where the Pacific moisture-laden wind could bathe its heights, there are ample evidences of former glacial action, but the type is that of the true mountain glacier, which can always be traced to its local source. . . . Not more than a thirtieth part of the entire surface of the fortieth parallel area was ever covered by glacial ice. . . . Whatever the greater causes may have been, the Cordilleran surface south of Washington Territory was free from an ice-sheet, and the only ice masses were small

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[3] Proceedings California Acad. of Nat Sciences, iii, 272.
[4] Id. 460, 461.

areas of local glaciers which did not cover two per cent. of the mountain country."

Whitney also tells us that in the interior parts of the Cordillera the ancient glaciers usually extended down to about 7000 or 8000 feet above the sea.[5]

In Dr. Wright's recently published "The Ice Age in North America," we read :—

" In the Rocky Mountain region and to the westward, there were formerly extensive glaciers in Montana, Wyoming, Colorado, Utah, Nevada and California, where now they are almost entirely absent. But the glaciation of this region was never general."

" According to Whitney there are no signs of ancient glaciers in Western Nevada, though some of the mountains rise to a height of 10,000 feet. . . . . In Colorado there are evidences of ancient glaciers *only above the* 10,000 *foot line.* . . . . The most southern point at which signs of local glaciers in the Rocky Mountains have been noted is near the summits of the San Juan Range, in South-Western Colorado. Here a surface of about twenty-five square miles, extending *from an elevation of* 12,000 *feet down to* 8000 *feet,* shows every sign of the former presence of moving ice. Northward of Utah and Colorado the signs of former glaciation are of the same local character; that is, glaciers everywhere radiated from the higher mountain masses, and extended *a short distance* down the cañons and valleys. . . . . The glaciers of the Sierra Nevada and Cascade Range in California, Oregon and Washington Territory were on a much grander scale than those in the Rocky Mountains." [6]

Mr. J. C. Russell says of the Sierra Nevada : " The evidence is such as abundantly to justify the conclusion that the ancient glacial system of the Sierra Nevada was local, and had no connection with a northern ice-sheet. The glaciers were clustered about and radiated from the higher portion of the range in the same manner as from the contemporary névé fields of the Wahsatch and Uintah Mountains." [7]

Professor George Dawson, in criticizing some of Prof. Whitney's views, says :—

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[5] United States Geol. Expl. of the 40th Parallel, pp. 459-461 and 464.
[6] Op. cit. pp. 148, 149.   [7] Eighth Report U. S. G. S. 327, 328.

" The general result arrived at in the areas of Whitney's and King's surveys is that comparatively only a very small portion of the highest ranges of mountains has ever been covered with glaciers, and that there has never been in this region anything like a northern drift period or a transportation of material in any given direction independent of the present topographical features of the country." [8]

Again, Mr. G. K. Gilbert, in his report on the geology of Nevada, Utah and Arizona, says :—

" About White's Peak in the Schell Range, Nevada, are the terminal moraines of five or six glaciers that descended to 8000 feet altitude in lat. 39° 15'. At about the same altitude and in lat. 39° are moraines and an Alpine lake upon the flanks of Wheeler's Peak, of the Snake Range of Nevada. Old Baldy Peak (N. lat. 38° 18'), near Beaver, Utah, overlooks two terminal moraines, one of which contains a lakelet at an altitude of about 9000 feet. No traces were seen of a general glaciation such as the Northern States experienced, and the cumulative negative evidence is of such weight that Mr. Gilbert is of opinion that the glaciers of the region referred to were confined to the higher mountain ridges." [9]

I will quote one more witness, and he the most important, because his remarks refer to the northern prolongation of the Rocky Mountains in latitudes where we should expect to see glacial phenomena on a very wide scale.

Professor George Dawson, the distinguished son of a distinguished father, has recently mapped out in detail the district threaded by the more northern parts of the Rocky Mountains, and what do we find on his map ? Why, that while on the west of the chain the erratics, which were once shed by the Cascade Range, come almost up to the Rocky Mountains on the one hand, and on the east the drift, which has everywhere spread so far from Labrador and its neighbourhood, has similarly travelled right up to the flanks of the same mountains, not an arrow or a mark is found on the map to show that the Rockies themselves threw off any erratics of their own, or that the great masses of ice which (one must suppose) covered them in the glacial age, if they were then

[8] Nature, 1881, p. 290.   [9] Id. vol. xii. p. 299.

existing, formed any barrier to the spread of drift on either side of them. In British Columbia and Vancouver's Island the glacial phenomena are more pronounced, but there also they point to a local accumulation of glaciers, due probably to former elevation of the country, and not to an ice-sheet.

The facts here stated are assuredly remarkable. They show very clearly that west of a certain line there is ample evidence that in the so-called glacial age the mountains nursed much larger glaciers than they do now, but there is no evidence of those wide-spread sheets of drift and the other associated phenomena which have been quoted as evidencing the work of ice-sheets. Here, as in New Zealand and Australia, we have ample witness to greatly enhanced local glaciers in former times, but not to any ice-sheets. Why is this? what possible reason can be adduced for it? why should the phenomena on the two sides of the Rocky Mountains be so contrasted, not merely in degree but in kind? I know of no rational explanation of the difficulty, except that in the country east of the Rocky Mountains the direct operation of the so-called ice age was supplemented by some other influences which did not extend to the country west of the mountains.

Let us now, however, turn to the district in America where the ice-sheet is supposed to have left its foot-mark.

A cardinal difficulty which such a theory has to face is the presence in the till of marine shells, and in Canada, as Dr. Dawson says, the presence of boulders with acorn-shells attached to them. It has been gravely stated in explanation of this, that the clay with the shells was pushed along by the ice-sheet which occupied the whole sea-bed of the North Atlantic, a view we have already criticized on mechanical grounds; but apart from this is the profound difficulty that the molluscs could only live in water, and could not exist under an ice-sheet which lasted for centuries.

Upon this point Dana writes : "The shell-bearing sea-beach deposits at Montreal and on Lake Champlain could hardly have been formed from a thriving growth of molluscs and other marine species, while the glacier lay over the whole region to the Gulf, a distance from Montreal of 500 miles. Whales (*Beluga Vermontana*) and seals, inhabitants of the bays of Arctic coasts, would never have made their way

beneath such a covering of ice to Lake Champlain, a region also under the ice." [1]

If the molluscs could not live in the sea under such conditions, no more could the land-plants on the land. We have seen how in some instances in Europe the *débris* of land-plants have been found in the glacial clays. This is more remarkably the case in America. In the leda clays of Canada are occasional nodules of calcareous matter, enclosing remains of plants. Among the latter occur *débris* of *Drosera rotundifolia, Acer spicatum, Potentilla Canadensis, Gaylussacia resinosa, Populus balsamifera, Thuja occidentale, Potamogeton perfoliatus, Equisetum scirpoides,* fragments of *carices* and *graminiæ,* and species of *fontinales* and *algæ.* These plants, says Dr. Dawson, occur in the marine leda clay containing its characteristic fossils, and were probably washed from the neighbouring land by streams. The climatic conditions pointed by these plant-remains is one somewhat colder than that now prevailing about Ottawa, and more like that of Southern Labrador, facing the Gulf of St. Lawrence." [2] All this is, however, assuredly most inconsistent with a great ice-sheet.

We also have in America the same evidence of the soft beds having drifted in more than one direction, that we have in Europe. Thus, one part of the deposit in Wisconsin and Minnesota, in the north-west, is characterized by a reddish colour, and by rock fragments from the region of Lake Superior, while above it and alongside of it is another deposit of a bluish colour, containing boulders and pebbles of limestone, and of cretaceous shale from the Red River. [3] This seems quite inconsistent with the operation of an ice-sheet. So does the so-called driftless region of South-western Wisconsin, an area of several hundred square miles in extent, surrounded on all sides by drift, and itself virtually free from it. I cannot understand how an ice-sheet which is supposed to have been capable of moving independently of the contour of the country, should have been so discriminating as to travel all round this particular area without overwhelming it.

The same conclusion is to be derived from the lobate character of the American so-called terminal moraines, which, as

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[1] Dana, Flood of the Connecticut River, 372.
[2] Canadian Naturalist, New Series, vi. 403, 404. [3] Wright, op. cit. 185.

Dr. Wright says, "clearly points to sub-centres of glacial radiation."

In America again we have, as on the continent of Europe, to account for the distribution of continuous beds of drift for hundreds of miles away from their parent rocks. As we have seen, we cannot attribute this portage to an ice-sheet which would be incapable of such work, and we must therefore supplement the action of land ice by some other force if we are to explain the phenomena.

Professor G. W. Dawson says : "I have not found, either in the Laurentian region, or over the area of the plains, or in the Rocky Mountains, any evidence necessitating the supposition of a great northern ice-cap, or its southern progress. . . . To reach the country in the vicinity of the 49th parallel, a northern ice-sheet would have to move up the long slope from the Arctic Ocean, and cross the second transverse watershed, then after descending to the level of the Saskatchewan valley, again to ascend the slope, amounting to over four feet in a mile, to the first transverse watershed and plateau of the lignite tertiary. Such an ice-sheet moving throughout on broad plains of soft, unconsolidated cretaceous and tertiary rocks, would be expected to wash the surface with broad flutings parallel to its direction, and to obliterate the transverse watersheds and valleys." [4]

"If it be supposed," says the same writer, "that a huge glacier, seated on the Laurentian axis, spread westwards across the plains, the physical difficulties are very serious. The ice, moving southward after having descended into the Red River trough, would have to ascend the eastern escarpment of soft, cretaceous rocks forming its western side, which in one place rises 900 feet above it. Having gained the second prairie steppe, it would have had to pass westward over its sloping surface, surmount the soft edge of the third steppe without much altering its form, and finally terminate over 700 miles from its source, and at a height exceeding the present elevation of the Laurentian axis by over 2000 feet. The distribution of the drift negatives the theory which would suppose the passage of an immense glacier across the plains." [5]

[4] Q. J. G. S. xxxi. 620.   [5] Id. 620.

If we examine the problem in America, as it presents itself at high levels, and not on the plains, we shall find that, as in Europe, the land ice moved along the direction of least resistance, and in conformity with the lines of drainage of the country.

Thus we read, " The facts collected by the late A. Murray, Director of the Geological Survey, show that in Newfoundland the striæ are quite divergent, following depressions and valleys in different directions. It seems probable, therefore, that here, as in Eastern Canada, local glaciers produced the chief striation observed, as pointed out by the late Captain Kerr, R.N." [6]

In regard to Prince Edward's Island, Sir William Dawson says :—

" These boulders (in Prince Edward's Island) being of rocks foreign to Prince Edward's Island, the question of their source becomes an interesting one. The majority are identical in mineral character with those occurring in the metamorphic districts of Nova Scotia and New Brunswick, at distances of from 50 to 200 miles to the south and south-west, though some of them may have been derived from Cape Breton to the east. Besides these boulders there are others . . . which must have been derived from the Laurentian rocks of Labrador and Canada, distant 250 miles or more to the northward. . . . Prince Edward's Island thus appears to have received boulders from both sides of the Gulf of St. Lawrence during the later post-pliocene period, but the greater number from the south, perhaps because nearer to it. It thus furnishes a remarkable illustration of the transport of travelled stones at this period in different directions." [7]

" It is a mistake," says Ramsay, " to suppose the striations in North America merely run from north to south, for Sir Wm. Logan, who has mapped them, proves that they often conform to the bends of the valleys." [8]

In regard to Canada, again, Mr. Robert Chalmers, of the Canadian Geological Survey, writes : " The glaciation of Eastern Canada has been effected by local glaciers on the higher ground, and drift-ice or icebergs on the lower coastal areas. . . . Each area of central depression has had its own

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[6] Geol. Mag. 1889, 214.   [7] Dawson, Geol. Mag. ix. 206, 207.
[8] Phil. Mag. 4th Ser. xxviii. 310.

glacier or glaciers. In Nova Scotia there was a shedding of the ice from the Cobequid Mountains northward and southward; and probably the elevation known as the South Mountain likewise sent glaciers down its slopes on either side. In New Brunswick the low watershed, running across it from north-west to south-west, sent off glaciers in opposite directions, or north-westwardly on the northern slope, and south-easterly on the southern, these courses being deviated from in a greater or less degree, however, according as the ice was influenced by local topographic features. In the Schickshock or Notre Dame Range in Quebec and its continuations, local circumstances have modified the prevailing direction of the striæ. While south-east is the prevailing direction in Acadia and New England, it is exceptional in the St. Lawrence valley, where the prevailing direction is south-west. Professor Hind has given a table of similar striation in New Brunswick, showing that the direction ranges from north 10° west to north 30° east in all except a very few cases. On the Blue Mountains, 1650 feet above the sea, it is said to be north and south, as in Nova Scotia north-west and south-east seems to be the prevailing course.[9]

On the Montreal Mountains, at a height of 600 feet above the sea, are huge boulders of felspar from the Laurenbell Hills, which must have been carried 50 to 100 miles from points of scarcely greater elevation, and over a valley in which the striæ are in a direction nearly at right angles with that of the probable driftage of the boulders.[1]

Bigsby traced the well-marked trap of Montreal up the St. Lawrence, to a point on the south shore of Lake Ontario, distant 270 miles from Montreal to the south-west. At the same time this rock had been carried southwards to the tract about Lake Champlain, in much greater quantity. The angle between the extreme lines of radiation from this point must be at least 50°.[2]

Sir William Dawson says : " In the low country of Cumberland there are few boulders, but of the few that appear, some belong to the hard rocks of the Cobequid Hills to the southward, others may have been derived from the similar hills of New Brunswick. To the southward and eastward of the

[9] Acadian Geology, 62.   [1] Id. 67.   [2] Q.J.G.S. viii. xli.

Cobequids, throughout Colchester, Northern Hants, and Pictou, fragments from these hills, usually much rounded, are the most abundant travelled boulders. In like manner, the long ridge of trap-rock extending from Cape Bloimdon to Briar Island has sent off great quantities of boulders across the sandstone valley which bounds it on the south, and up the slopes of the slate and granite hills to the southward of this valley. Well-characterized fragments of trap from Bloimdon may be seen near the town of Windsor; and I have seen unmistakable fragments of similar rock from Digby Neck, on the Tusket river, thirty miles from their original position. On the other hand, numerous boulders of granite have been carried to the northward from the hills of Annapolis and deposited on the slope of the opposite trappean ridge; and some of them have been carried round its eastern end, and now lie on the shores of Londonderry and Onslow. So also, while immense numbers of boulders have been scattered over the south coast from the granite and quartz rock ridges immediately inland, many have drifted in the opposite direction, and may be found scattered over the counties of Sydney, Picton, and Colchester. These facts show that the transport of travelled blocks, though it may here, as in other parts of America, have been principally from the northward, has by no means been exclusively so; boulders having been carried in various directions, and more especially from the more elevated and rocky districts to the lower ground in their vicinity. Professor Hind has shown the existence of a similar relation between the boulders of New Brunswick and the hilly ranges of that country.[3]

The dominant direction of the striæ in the valley of the St. Lawrence, along the high lands to the north of it, and across western New York, is north-east and south-west. There is another series of scratches, running nearly at right angles to the former, across the neck of land between Georgian Bay and Lake Ontario, down the valley of the Ottawa, and across part of the Eastern Townships, connecting with the prevalent south and south-east striations which occur in the valley of the Connecticut and Lake Champlain and elsewhere in New England, as well as in Nova Scotia and New Brunswick. What were the determining conditions of these two courses, and

[3] Acadian Geology, 60, 61.

were they contemporaneous or distinct in time ? The first point
to be settled in answering these questions is the direction of
the force which caused the striæ. Now I have no hesitation
in asserting, from my own observations as from those of others,
that for the south-west striation the direction was from the
ocean towards the interior, against the slope of the St.
Lawrence valley. The crag-and-tail formation of all our
isolated hills, and the direction of transport of boulders carried
from them, show that throughout Canada the movement was
from north-east to south-west. "*This,*" says Sir William
Dawson, "*at once disposes of the glacier theory for the prevail-
ing set of striæ.*" [4]

Let us now go somewhat further south.

In a paper by Professor Winchell on "Some Indications of
a Northward Transportation of Drift Materials in the Lower
Peninsula of Michigan," he calls attention to the occurrence
of masses of corniferous limestone, such as is found *in situ*
along the western shores of Lake Erie and at numerous
points throughout the county of Munroe, in the southern
parts of Michigan. These blocks are found resting upon the
Hamilton group, the Marshall group, the Carboniferous lime-
stone, and probably also the coal measures. Winchell,
speaking of these limestone erratics, says :—"There are
insuperable objections to assuming that they have been trans-
ported with the great mass of drift materials from the
northern outcrops of the rocks of this age at Mackinac and
the surrounding region. *First,* the transporting agency has
not moved masses of other kinds of rocks which attain to
anything like the same dimensions. *Secondly,* that agency, if
we may judge from the condition of the siliceous, trappean,
and gneissoid boulders of admitted northern origin, would
have ground to powder so fragile and friable a rock as these
limestones ; or at least would have broken them into small
fragments, and deposited them in a worn and rounded condi-
tion. *Thirdly,* if the corniferous limestone could have been
transported in such masses from its northern outcrops to
Southern Michigan, much more would the harder and more
massive Niagara limestone of the same regions have been
similarly transported. The same may be said of the Trenton

[4] Dawson, Acadian Geology, 69.

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limestones. We find, however, that fragments of these lime-stones are of rare occurrence; and the fossils of Silurian age scarcely sustain to those of the corniferous limestone the ratio of one to one hundred, in the drift deposits of the southern portion of the State. Even the Carboniferous limestone, whose outcrop extends through Kent, Eaton, Jackson, and Oakland counties, is scarcely represented among the drift materials of the region in question. It is apparent that we must look in another direction for the origin of these lost rocks.

"The nearest outcrop of the corniferous limestone is in Northern Indiana and Ohio, and the south-eastern corner of Michigan. Every fossil cited above is found in place in some part of that region. The formation dips under the peninsula of Michigan; and, throughout the area occupied by the lost masses in question, it lies from one hundred to twelve hun-dred feet beneath the surface—the depth, of course, increasing towards the centre of the Carboniferous area. The circum-stances suggest the exertion of some powerful agency acting northward with tremendous energy, but with a gentle and equable movement. . . .

"Additional facts exist which seem to lead the mind further towards a belief in a northward acting post-glacial agency. The fossils of the Hamilton group, whose outcrop is along a belt lying somewhat further north than the Corniferous lime-stone, are scattered through the soil of the region lying still further north, in a degree of abundance which bears about the same ratio to that of the corniferous fossils as the attenuated Hamilton rocks bear to the corniferous." Winchell, after enu-merating some of these fossils, continues :—"These species exist in an admirable state of preservation, quite incompatible with the theory of their transportation from the far north; and they occur across a belt of the state reaching at least thirty miles north of the outcrop of the Hamilton rocks.

"Again, above the identifiable Hamilton rocks, we find, in this State, a great thickness of argillaceous and bituminous shales, destitute of fossils, but freighted with kidney iron ore. Nodules of this ore are strewn not only over the region of the outcrop of these "Huron" shales, but throughout Wastenaw and other counties lying over the upper Devonian and lower Carboniferous strata—to say nothing of the occurrence of such

nodules within the limits of the coal measures, where, by some geologists, they might preferably be referred to the indigenous strata. Still again, the well-marked fossiliferous beds of the Marshall sandstone, lying next above the Huron shales, and outcropping along a belt still further north, are represented by a series of enormous fragments resting over the non-fossiliferous upper portions and the Carboniferous limestone. The lower or fossiliferous portions of this formation do not outcrop further north than Moscow in Hillsdale county, while fragments of it have been transported in great abundance into the southern townships of Jackson county. . . . The fossiliferous layers of the Marshall sandstone are decidedly friable—inasmuch that it is in little request for building purposes—and it would seem absurd to suppose that these large fragments had been moved two hundred miles from the northern outcrop of the formation, when a transfer of ten to fifteen miles from the southern outcrop would bring them to the position which they occupy. We should expect, also, if derived from the north, that some contrast in the organic facies, due to local, if not to climatic causes, would present itself; but on the contrary, we find the fauna of the fragments strictly identical with that of the nearest indigenous rocks of the same age." [5]

Mr. Winchell also refers to observations made by himself in Alabama, especially in the neighbourhood of the junction of the " rotten limestone " of the Upper cretaceous with the clay and sandy strata of the Lower cretaceous. He says, " The red loam of the central belt of the State, which I have proved to be the 'rotten limestone' altered *in situ* or with slight transportation, has in many cases along the junction of the upper and lower strata been moved *northward* over the clayey and sandy region appertaining to the lower cretaceous." [6]

Mr. Henry D. Rogers in his " Geology of Pennsylvania " distinctly says, " The striæ there, instead of being parallel, cross one another at various angles." He moreover says that " they are not rigorously straight, but curve slightly to conform to inequalities in the shape and hardness of the resisting surface." [7]

Dr. Wright tells us how the system of interior moraines

[5] Op. cit. 333-338.   [6] Id. 338.   [7] Op. cit. 775.

points to independent ice movements in the country west of the great lakes rather than to a great ice-sheet. Thus, he says, President Chamberlin, writing of Wisconsin, says that "the ice movement through Green Bay was in a measure independent of that through Lake Michigan. . . . It appears from the conclusions of Mr. Upham Chamberlin and Professor Chamberlin that in the north-west the ice advanced in four lines of motion, pointing to a centre a little below Dubuque in Iowa. One line of advance was down the depression of Green Bay in Wisconsin . . . a second line of movement was down the valley of Kewanee Bay . . . the third movement was along the line of the main axis of the western end of Lake Superior, across the Mississippi, past Minneapolis as far as Lake Minnetoaka . . . the fourth movement was from the region of Lake Winnipeg in the Red River Valley towards the south and south-east," and he speaks of certain districts where these ice-streams met as "an interesting battle-ground of the glacial forces." [8]

On a later page Dr. Wright describes a number of moraines pointing to local glaciers in the State of Maine, etc., and he says: "Dr. Dawson's surmise as to the significance of the Missouri coteau, President Chamberlin's theory as to the meaning of the Kettle range, Professor Cooke's delineation of the moraine across New Jersey, and Clarence King's interpretation of the glacial accumulations on the south shore of Massachusetts, were in the minds of the authors of independent origin." [9]

Lastly, the interesting maps given by Professor Wright on pages 175 and 392, showing the direction of the striæ and of the boulder drift in North America, point unmistakably to there having been there, as in Europe, a number of independent glacial centres—point, in fact, to phenomena similar to, and to be explained by, the *modus operandi* of existing glaciers. There, no doubt, as elsewhere, this, the true handiwork of ice, was supplemented by the action of another force. To the consideration of the problem of what this supplementary force was, we shall turn in the next chapter.

[8] The Ice Age of North America, 184-185.　　　[9] Id. 193.

CHAPTER XVII.

WE MUST SUPPLEMENT THE ACTION OF ICE BY THAT OF WATER
ACTING IN SOME OTHER WAY THAN THE DIURNAL OPERATIONS
OF SEAS, LAKES, OR RIVERS, IF WE ARE TO EXPLAIN THE
PHENOMENA OF THE DRIFT.

" Après avoir voulu tout expliquer par l'action de l'eau, on voudrait
à présent trouver dans l'activité de la glace une réponse à toutes les
questions. Il faut être moins absolu."—FÂLSAN, "La période
Glaciaire," 125.

Glaciers alone will not explain all the drift phenomena—We must
supplement the action of ice by that of water if we are to explain
them—The so-called drift phenomena involve two stages—
Durocher—Aqueous origin of the boulder-clay—Milne Home,
Brown, Bonney, Nordenskiold, Darwin, Dawson, Bulman—Shells
and rounded sand-grains in the Till—Geikie, Reade, Dawson—
Shells and ripple-marks in the glacial sands point the same lesson
—So does the sifting of the materials of the so-called glacial beds
—So does the stratification of many of these beds—So does
the peculiar arrangement of the beds—A. Geikie—Kames—A.
Geikie, Lyell, Phillips, Milne Home, Green—Eskers—Jenkins,
Kinahan—Asar—J. Geikie, Nordenskiold—Horsebacks—Agassiz,
Dawson—Crag and tail—Distribution of the stratified and shelly
drifts — Russia — Murchison—Scandinavia—D'Archiac, Kjerulf,
Erdmann—Denmark—Murchison—North Germany and the Low
Countries—Geikie—Central Europe—Agassiz—Scotland—J.Geikie,
Jamieson—England—H. B. Woodward, T. Mellard Reade, Darwin,
Crosskey—Ireland—Jukes, Kinahan — North America — Desor,
Dana—Causes assigned for the stratified drift—Fluviatile origin
discussed—Agassiz, Murchison, Ramsay, Jamieson—Subglacial
streams—Agassiz, Charpentier, Wright—Lacustrine origin—J.
Geikie, Sedgwick, Jamieson — Marine submergence — Trimmer,
Ramsay, Phillips, Jamieson, A. Geikie—Objections to this theory
—Jamieson, Scouler, Dugald Bell, Lyell, J. Geikie, T. M. Reade,
Belt, Bell, Darbishire, Maw, E. Forbes, Mackintosh, Dana.

In the two previous chapters I claim to have shown, first
that the theory of ice-sheets is directly at issue with the
known physical qualities of ice, and secondly that it is not
supported by the geological evidence which everywhere
points to the former occurrence of local glaciers on a wide

scale, moving as modern glaciers do, and not to trans-
cendental ice-sheets moving in a way in which ice is nowhere
now seen to move.

It would be utterly misleading, however, to suppose that
when we have postulated the existence of these "glorified
glaciers," we have explained all the phenomena of the drift.
We are still face to face with the same difficulties which first
embarked men in the pursuit of the glacial nightmare. We
have shown, it is true, that ice-sheets are transcendental; but
we have not dissipated the difficulties which made Schimper
and Agassiz and others argue that glaciers alone will not
explain everything, and, it seems to me, we are bound to
face these difficulties and to try and find a rational explana-
tion of them if we are to solve the most interesting riddle
in geology.

Glaciers will not explain the existence of the drift spread
out, not in the form of moraines, but in continuous sheets
stretching over hundreds of miles of level country. Glaciers
cannot, it seems to me, explain the separation of the drift
into beds of gravel, of sand, and of clay; the known *débris*
of glaciers are mixed and heterogeneous, and not separated
and sorted in this fashion. Glaciers cannot explain the
mounds called eskers, kames, or whalebacks; nor the long
dykes called äsar by the Swedes, which in many cases are
partially stratified, nor the partial stratification, the false
bedding, and the huge curving lines of deposit which the
sands and fine gravels of the drift series often assume.

By no reasonable process can we explain the existence of
marine shells in glacial beds, sometimes at low levels, some-
times at very high ones, on the theory that these beds have
been deposited by glaciers radiating from high mountains
over the low country. Nor, again, can we by this theory
account for the moving of erratics and boulders from lower
levels to higher ones, as can be shown to have occurred in
numerous instances. Nor, again, can we thus explain the
presence of boulders of Scandinavian origin in the drifts of
the East of England. By invoking glaciers on a larger scale
we do not explain, again, the fact that the drift of the boulders
and the direction of the striæ on the rocks are not always
consistent with one another. Nor the occurrence of cross-

hatching among the striæ. Nor, lastly, the fact that the striæ occur in many cases in apparently continuous lines traversing a wide expanse of country, and utterly disregarding its contour and drainage.

These stubborn facts will not conform themselves to a theory which merely invokes glaciers on a larger scale to explain the facts of the Drift period.

If we turn from the facts which can be rationally explained by the action of ice working on lines and in a method consistent with its known properties, to the facts I have enumerated as inconsistent with the handiwork of ice, it seems to me that we are compelled by the conditions of the problem to revert once more to those arguments and lines of thought which the extremer advocates of the Glacial theory have put into the shade, and to invoke the action of water on a wide scale as alone capable of explaining the difficulty. Water is, it seems to me, the only agency in Nature which is capable of explaining *some* of these facts. This has been universally admitted from the days of Agassiz onwards, and by the champions of every form of the Glacial theory; the difference between them depending more upon the extent of the operations to be attributed to water than upon the fact itself.

What I wish to press home in this chapter is that the influence of water in distributing the drift has been much greater than some geologists would allow, and I would attribute to its wide-spread action subsequently to the existence of the great glaciers the explanation of the various phenomena I have above enumerated, which cannot be explained by ice acting in a normal way.

In common with many other inquirers, I would postulate for the explanation of the so-called glacial phenomena, a double process—one glacial, the other aqueous. The existence of two stages in the history of the Drift is no new discovery. It was hinted at by Charpentier and Agassiz; but the first writer known to me who clearly saw that the phenomena in the North necessitate our postulating two processes—two stages and not one—was Durocher.[1] It is true that he was entirely wedded to

[1] Mémoire sur les phénomènes diluviens dans le nord de l'Europe.

aqueous theories, and had not grasped the fact that ice in the shape of glaciers had done a great deal of the work. He nevertheless saw that we must invoke a double process if we are to explain the facts. To one of these processes he assigned the formation of the striæ, the polishing of the surfaces, and the deposition of the äsar ; and to the other, the deposition of the drift with the boulders which it contains ; and he placed the former process before the latter, since the polished and striated rocks are so frequently overlaid by the drift, while he considered that the violent force necessary to carve the rock surfaces must have been different in character to that which deposited the glacial sands, etc., in successive layers. The former he attributed to a great diluvial wave which issued from the Polar Sea, the latter to the action of the sea when the country was generally submerged, which he considered also explained the presence of the well-preserved marine shells often occurring in the stratified beds. In order to explain the presence of large blocks and erratics in the latter, he invoked icebergs and floating ice.[2]

Upon one point Durocher's induction seems to have been faulty, namely, in attributing the äsar to the same cause which made the striæ. The äsar, as we shall see, are themselves partially stratified and contain marine shells, and belong to the second and not the first stage of the Drift phenomena. He was apparently misled by the fact that in Scandinavia the ärar frequently have the same direction as the striæ. In Finland, however, this is not so much the case. Where it is so, as M. d'Archiac says, it does not imply the contemporaneousness or community of origin of the two phenomena, but only points to the fact that the contour of the land having been the same when the two phenomena occurred, the general direction of the two have been dominated and controlled by the same lines of country.[3]

With this exception it seems to me that the separation of the phenomena by Durocher is amply justified. The point where most students would now differ from him is in attributing the polishing and striating of the rock surfaces in mountain districts to diluvial action. This has been amply

[2] Comptes Rendus, xiv. 78-110.   [3] Histoire des progrès, etc. ii. 37, 38.

shown to have been the work of glaciers in days when glaciers were more developed than they are now.

Let us now examine some of the features of the so-called glacial beds which seem to point unmistakably to the action of water, and first in regard to the materials of the drift. The most remarkable of these is doubtless the till or boulder-clay which occurs so commonly in Britain, in North Germany, and North America. It is the universal experience of observers that nothing like till is being formed by glaciers.

On this subject Mr. Milne Home says : " If boulder-clay is formed, as is alleged, by the action of glaciers ; if it consists of *débris* derived from the rocks, which the ice grinds down in its passage over them, and which are pushed forward by its ice foot, we should see boulder-clay now forming in those countries where glaciers are in action. But it has never been alleged that in Switzerland, Norway, or Upper India, whose glaciers have been described by competent observers, anything like a boulder-clay is seen to be produced. I have twice," he adds, " been in Switzerland ; and, being anxious to watch the effects of glaciers on the rocks, made them a subject of study, and penetrated under three glaciers near their lower extremities, without discovering anything like boulder-clay. Great abrasion of rocks there was undoubtedly. Blocks and pebbles under the ice I saw in abundance, all grinding, and many of them scoring the rocks. Much sediment there was, flowing out from under the ice. But what became of this sediment ? It was carried off into rivers and lakes, there to form beds of mud or sand—none having any resemblance to boulder-clay. The terminal moraines of glaciers, no doubt, resembled it in one feature—want of stratification ; but the absence from these moraines, of clay, hard, tough and compact, showed that the deposits were essentially different. It is no small confirmation of my own testimony on this point, that Agassiz, when he visited Scotland to search for the signs of ancient glaciers, avowed that he had never seen boulder-clay before he saw it in Scotland." [4]

Again, Mr. Brown says : " In the Alps people can easily

[4] Trans. Roy. Soc. Edin. xxv. 659.

get some length under glaciers, but never find till. On examining old tracks of glaciers they find large mounds of moraine *débris*, but not boulder-clay." [5]

Mr. Bonney, speaking of the Swiss glaciers which he knows so well, says : " In no case have I been able to find signs of any deposit resembling till or boulder-clay. . . . In short, the result of an experience of some years has convinced me that if anything like the till or ground moraine of recent geologists exists in the Alps, it is a very local and exceptional phenomenon." [6]

While there is apparently no evidence that till or boulder-clay is ever produced by land ice, Mr. Brown, who examined some of the problems of glaciation in Greenland, is satisfied that a clay precisely like the boulder-clay is being now deposited in the Greenland fiords. He tells us that when the milky stream that comes from underneath the glaciers debouches into a fiord, it discolours the water for miles, finally depositing on the bottom a thick coating of impalpable powder. When this falls in the open sea, it may be scattered over a considerable space ; but when, as in most cases, it falls in narrow, long fiords, it collects at the bottom, shoaling up these inlets for many miles from their heads, until in the course of time the fiord gets wholly choked up. Mr. Brown quotes a distinguished Scandinavian explorer as having affirmed that the boulder-clay near Edinburgh is very like the deposit now being formed in the Spitzbergen fiords. In this clayey bed, he goes on to say, the Arctic molluscs and other marine animals find a congenial home, and burrow into it in great numbers ; and he explains how these molluscs always live near the surface, so that if a catastrophe were to happen by which they were killed, the lower layers of the clay would prove to be barren and only the upper ones containing shells. If, he continues, we were able to examine the bottom of the Arctic Sea (Davis Straits, for instance), it would be found that this clayey deposit would not be found over the whole of it, but only in patches ; for instance, all the ice fiords would be full of it, to the depth of many feet, and shoaling off at the seaward ends, and certain other places on the coast would be also covered with it ; but the middle and mouth of Davis Straits and Baffin's Bay

[5] Brown, Trans. Ed. Geol. Soc. ii. 389.   [6] Geol. Mag. xii. 197.

and the wide intervals between the ice fiords would either be bare or but slightly covered with small patches from local glaciers.[7]

Similarly, Nordenskiold says, speaking of the clays now being formed in Greenland : " The material of the clay-beds has evidently been deposited by the glacier rivers, whose muddy water everywhere burst out from under the inland ice ; but in general the deposits are sea formations, i.e. they have been deposited under the level of the sea." [8]

Similar views as to the origin of the till were propounded by Darwin. Speaking of the till in Anglesea, he says : " I cannot doubt that this till was accumulated in a sloping sheet beneath the waters of the sea. In composition it resembles some of the beds of till in Terra del Fuego, which have undoubtedly had this origin." [9]

Sir William Dawson urges that the boulder-clay is a submarine and not a subaerial deposit, which, he says, is shown by the fact that many of the boulders of the native sandstone are so soft that they crumble immediately when exposed to the weather and frost.[1]

I agree with Mr. Bulman when he says that the absence of lamination in boulder-clay is no proof that it was not deposited by water, since laminæ are caused either by an intermittent supply of sediment or the presence of flat plates of mica ; and when the ice produces a continuous supply of rock-flour all the year round, there is naturally no lamination, while flat plates of mica are also naturally absent.[2] This subaqueous origin for the till accounts for its local distribution, generally speaking, in maritime and not in inland districts.

Shells also occur in the boulder-clay, which point clearly to its subaqueous origin. These have been described from several localities. A list of such of the localities in Scotland as were then known is given by Mr. Geikie.[3]

The fact has been frequently observed elsewhere in Europe, and the same is the case on the other side of the Atlantic, as at Isle Verte, Riviere du Loup, Murray Bay, and St. Nicholas on the St. Lawrence, and also at Cape Elizabeth, near Portland,

Q. J. G. S. xxvi. 686-7.   [8] Geol. Mag. ix. 410.
[9] Ed. Phil. Journ. xxxiii. 1842, 357.   [1] Handbook of Geology, 154.
Geol. Mag. Decade III. ix. 310.   [3] Great Ice Age, 559-563.

where there are tough, stony clays of the nature of true till, and the lower part of the drift contains numerous sea-shells of the usual post-pliocene species.[4]

The occurrence of these marine remains seems to be consistent only with the boulder-clays, or some of them, having been deposited under water.

The same conclusion must be drawn from a large proportion of the boulders. They are for the most part clearly water-rolled and water-worn stones and pebbles, and could only have been rounded by the action of water, the stones deposited by glaciers having generally flattened sides, and being striated. Especially obvious is this the case with the pebbles in the gravels of the glacial beds.

On this subject Mr. Hopkins says what is unanswerable. " Blocks," he says, " cannot generally be rounded by attrition when floated on icebergs, or carried on the upper surface of a glacier. A small portion of those brought down by glaciers are rounded by being rolled between the ice and sides or bottom of the glacial valley, but this is a rough grinding, and all the specimens I recollect to have examined immediately at the termination of a glacier, wanted that more perfect smoothness of surface which distinguishes a water-worn boulder. It might be contended that blocks floated on icebergs might be rounded and polished before being taken up by the ice, or after being deposited by it. If such were the case, the effect must be produced either on beaches by the action of breakers, or at the bottom of the sea by that of currents. The action of breakers on large blocks, however, as far as my observation has extended, rarely tends to give to them a rounded form, but, on the contrary, to wear them into very irregular shapes, till they are so reduced in magnitude as to be rolled about by the force of the waves; the most prominent points then become subject to the greatest attrition, and the surface afterwards assumes that form and polish which distinguishes a water-worn boulder."[5]

Mr. Mellard Reade draws the same conclusions from the rounded grains of quartz forming the sand in the till. He says that river sands are generally little rounded, while sea sand is very much so: and he adds, "When we find these highly-

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[4] Acadian Geol. 68.   [5] Trans. Camb. Phil. Soc. viii. 238.

polished grains mixed up with angular and little-worn fragments of Snowdonian rocks, forming the bulk of the till of the coastal plain, it is evident that they are of sea origin, and serve as certainly as the fragmentary shells of mollusca to declare this fact;"[6] and he tells us that at Moel Tryfaen he found, on washing the till lying at a level of 800 feet above the sea, between "The Rivals" and Mynydd Carnguwch, that it contained highly polished and worn quartz grains.[7]

What is true of the boulder-clay is true in a larger degree of the so-called glacial gravels and sands, namely, that in many maritime districts they contain remains of marine molluscs, pointing, as it seems unmistakably, to their having been deposited under water. I shall refer at some length to these shelly gravels and sands presently.

I would here call attention only to one feature of the sands. As evidence that they were deposited by water, and not by ice, may be mentioned the ripple-marks occasionally found in them. Mr. Mellard Reade read a paper on these marks before the Geological Society, and has published some sections. He figures one case from Ketley, near Wellington, Shropshire, and says : "I measured one of the ripple-marks in bed number 1, and found it was nine inches from crest to crest, and $1\frac{1}{4}$ inches high; the fall of the wave indicated that the wind producing it was from the N.W." In 1883 he tells us he found at Tranmere, in Cheshire, ripple-marks in hard, fine, loamy brown sand, forming the flank of what must have been a submarine bank, and covered with a compact mass of boulder-clay many feet thick. In the sand were fragments of marine shells. Mr. Reade quotes Mr. Mackinloch as saying that a sand-bed at Egremont, in Cheshire, presented the appearance of having been finely ripple-marked immediately before the tranquil deposition of an inch in thickness of leaf-like laminæ.

Mr. Jukes Brown states that at South Ferriby, in Lincolnshire, a bed of indurated sand, a few inches thick, is beautifully ripple-marked on the upper surface, which underlies the boulder clay. Mr. Searles Wood[8] says, " there is a similar bed below the Hessle clay in the Hessle quarry. "

Mr. Lamplough said "ripple marks are by no means

uncommon in cliff sections about Bridlington, being generally
in beds of sand and sandy clay, interstratified with the purple
boulder clay." He gives an example of one of them.[9]

Mr. Clement Reid said "that on the east coast of Norfolk
ripple marks are of common occurrence in drift deposit."[1]

Let us now turn from the contents of many of the glacial
beds to their internal arrangement and structure. Wherever
we see ice at work, whether in the shape of glaciers or icebergs
or of coast ice, we shall fail to find that it has any capacity for
sifting out the various ingredients of a mass of soft materials,
classifying them into different deposits and arranging them
in successive layers or strata. Ice acting upon soft materials
pounds and kneads and mixes and crushes, but it cannot and
does not sift materials. The moraine of a glacier is a very good
sample of its handiwork. Its materials are heterogeneously
mixed boulders and sand and mud. Wherever therefore we
find signs of sifting, we may be absolutely certain that some
other agent than ice has been at work, and I know of no agent
but water which is capable of this kind of work.

It is a curious fact that whereas we have in the interminable
literature dealing with the drift, abundance of discussion as to
the separation of the glacial epoch into several stages by
means of beds of clay intercalated with beds of sand or gravel,
there is hardly a word of explanation forthcoming of the
phenomenon of separation itself. How has it come about
that these drift beds are sifted in this fashion ? that we should
have thick beds consisting entirely of clay, sometimes red,
sometimes blue, sometimes chalky, but containing no sand
or gravel, and merely containing a large number of glaciated
or smoothed boulders, that elsewhere we should have wide-
spread beds of clean sand, elsewhere again sand mixed with
boulders of different sizes, but containing no clay ? How again
comes it that these beds of sand and gravel should be inter-
calated with and yet sharply defined from the beds of clay,
and should at other times occur in pockets and lenticular
masses in the clay itself surrounded by it on every side ? This
not merely locally, but in many latitudes in America as well
in Europe. Not merely on the sea-board, but far inland, as in
our Lancashire upland valleys, and over the wide plains of

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[9] Geol. Mag. Sept. 1879.   [1] Q. J. G. S. xl. 267-269.

eastern England. These beds are as different as they can be to any possible glacier deposits, which are always heterogeneous and never assorted in this fashion. Even if we could assign a different age to these several beds, as some geologists argue, we should still have to account for their being sifted.

Water alone, it seems to me, could do this work, taking up and redepositing soft materials according to their specific gravity, washing sand and gravel in one direction to make sand banks as we see it doing now, and depositing the more impalpable mud in beds of clay or brick elsewhere.

What is true of the sifting of the earth and clays is still more true of the stratified character of the beds in many places. Stratification can only be the result of water action. Ice acts directly contrary to any process of stratification, whether as an ice-foot or as a glacier. It pounds and kneads the beds over which it travels into a hotch-potch of mixed materials, and cannot separate them into laminæ and stratified layers, least of all can it twist and turn these laminæ into beautifully continuous and perfect curves, sometimes recurved upon themselves like those occurring in the Cromer cliffs and in many places elsewhere. This must be due to water, and has, in fact, been attributed to water by many ardent champions of a glacial age, some of whom have separated these stratified drifts and treated them as remanié by water, and have also referred to them as glaciated drifts. These stratified beds occur in many countries, and they occur at many altitudes, and overlay the more local beds of a heterogeneous character, and overlay also the polished and striated rocks where the old glaciers marched, and they are very widespread. For the most part, in the interior of the country they contain no traces of life, but as we near the coasts they are found to contain shells and other remains of marine animals.

It is not merely the sifting into different kinds of deposit and the stratification, but the peculiar way in which the beds of sand and gravel often occur in the clays, which is quite inconsistent with the steady and unchangeable action of ice, and consistent only with the more versatile action of water.

Sir A. Geikie quotes many cases from Scotland of the occurrence of irregular nests and lenticular laminæ of sand, sometimes curiously contorted, intercalated with the till. In one

case, in a cliff of boulder clay near Hawick, there was a zone of water-rolled shingle overlaid by a few inches of well-stratified sand, silt and clay. The stones were identical with those in the boulder clay, except that they were more rounded, and resembled in every respect the stones now lying in the bed of the Seltrirg, and he says that they suggest the operation either of a river or a lake.[2] On the banks of the Carmichael water was another section, in which one of the beds of the stratified series was a coarse shingle, where the insterstices between the stones were occupied by fine clay and sand, and they resembled " the intermittent sediments of a stream than any other form of aqueous deposit." [3] At Airdrie was found a bed of brick-clay, intercalated with the till, containing numerous shells of *tellina proxima.* Usually the specimens were broken, but a good many were taken out entire with both valves together. The place was computed to be 560 feet above the sea coast, and was the highest in Scotland from which drift shells have come.

Near the same place Sir A. Geikie examined some underground excavations made in sand underlaying boulder clay, which he says contained frequent nests of sand of extremely limited extent, lying in irregular hollows in the till, the stratification being well shown, sometimes conforming to the sides of the enclosing basin, and sometimes lying against them. Below this till lay a group of finely-stratified sands and clays, varying in thickness up to twenty or thirty feet. These sands contained endless lenticular laminations of extremely fine olive and pale drab-coloured clay, from the thinness of a sheet of paper up to beds a foot or more in depth. "It is hardly possible to convey an adequate idea of the minuteness of the alternation and interlacing of these sand and clay seams. They are full of false bedding, the different layers being marked by a succession of darker and lighter materials. In the clay these alternations are as fine as the lines of a wood-cut. Indeed, when one of the miners pared away, with a sharp knife, a clean surface of one of the clay bands, the sectional view laid open rather resembled a reduced diagram, or even a photograph of a great cliff of false-bedded, jointed and panelled strata, than the occurrence of these structural

features in nature. . . . The line of demarcation between the coarse unstratified till and the finely laminated clays and sands is always sharply defined." " In one place it appeared as if the till had been to some extent injected between and among the layers of clay and sand, which were there short and irregular." [4] " Immediately under the till the beds of clay and sand are sometimes folded over each other like a flattened form of the letter S, after which they continue irregular as before." [5] In regard to the cause of these deposits, Sir A. Geikie argues that it must have been during a pause in the deposition of the clay, when a run of water would find its way along its inequalities.

Let us now turn to the way in which the stratified drift is found. In general it consists of either continuous beds or of terraced masses, or again of long ridges or mounds. We will consider each in turn.

In regard to the continuous mantles of sand or gravel or clay, nothing could well be more different than they are to any deposits of glaciers. Moraines are heaped up in concentric mounds, or consist of piles of great stones, nor could glaciers spread out beds with undulating surfaces and smoothed down outlines. This spreading out seems unmistakably to be the result of water action.

In other cases the gravels and clays occur as terraces on the sides of valleys and on the flanks of hills; terraces of rolled pebbles and smoothed stones mixed, are intercalated with sand, and in some cases with angular *débris*.

Professor Geikie describes the terraces of gravel at Eaglesham as being at a height of 800 feet above the sea level. On the Moorfoots he had traced others up to a height of 1050 or 1100 feet. Still further south, high level shelves of gravel and sand were found by Mr. Skae in Nithsdale, at a height of 1250 feet above the sea, while Mr. Jamieson [6] has enumerated several instances of the occurrence of stratified gravels, clay and silt at considerable heights in the Highlands. In Perthshire shelves and terraces of gravel occur at a height of 1050 to 1100 feet. [7]

The most famous of these terraces are those which fringe

[4] Id. 62.   [5] Id. 63.   [6] Q. J. G. S. 1865, p. 177.
[7] Great Ice Age, 248-9.

the slopes of the valleys in the district of Lochaber, and which consist chiefly of angular shingle. In Glen Roy there are three distinct shelves, 856 feet, 1065 feet, and 1149 feet respectively above the sea level. I shall revert to these terraces later on.

Similar terraces exist in America, and Professor Hitchcock has described them as existing at a height of 2449 feet in the White Mountains.[8]

Such terraces as are here referred to are quite different to the lateral moraines of glaciers, or to other similar handiwork of ice, while they are exactly the kind of deposits left by water, which leaves such margins as the footmark of its highest level.

Thirdly, and this must detain us longer, these beds, while generally retaining a smooth and even contour, swell up into mounds and ridges in places. These are variously known as kames, eskers, hogbacks, äsar, etc., etc.

"A kame consists," says Sir A. Geikie, " almost entirely of sand or gravel, varying in texture from the finest grains up to a coarse aggregate of boulders. These materials, except in rare cases, are stratified. They often alternate with each other in rapid succession; the layers or laminæ of sand are intercalated in the gravel beds, and occasional seams of gravel vary the stratification of the well-bedded masses of sand. The sands, moreover, are, as a rule, full of diagonal lamination or current bedding. *It seems plain*, therefore, that currents of water must have played some part in the accumulation of these sedimentary deposits. This is still further shown by the occasional introduction of lenticular seams of laminated clay."

He distinguishes two kinds of kames as occurring in Lanarkshire, one made up wholly or nearly so of coarse gravel, and the other of fine sand and gravel. The shingle kames are usually sharper and higher in outline than the others. Their component gravel is either stratified very rudely or not at all. The stones are usually well-rounded, and vary in size up to blocks four or five feet in length, which in one or two instances were found to be polished and striated. The striæ were less sharp, however, than those on stones in the boulder clay. In a typical instance, near Carstairs, the mass of coarse gravel

forming the kame is overlaid on its south side by a series of finely-laminated clays and sandy beds, while in another place it contains here and there thin layers of sand, and at least one lenticular seam of fine olive-coloured clay, and he suggests that the huddling tumultuously together of the gravel, with here and there nests and layers of stratified sand and clay, was due probably to complex causes, involving not only the tides and currents of the sea, but of drifting ice.

The second kind of kame consists of mounds of sand or of sand and gravel in well stratified beds. These strata present a convex or dome-shaped arrangement ; they are heaped up over a central nucleus of the same materials, and it is the upper surface of the outer bed which defines the outline of the kame. When the outer bed consists of a layer of gravel, the ridge is usually sharper and better preserved than where loose sand has formed the surface. These beds of clay, sometimes finely laminated, are occasionally to be observed, even among the densest masses of sand and gravel. . . . Sir A. Geikie, while attributing the deposition of these kames to water, argues that the stratification and diagonal lamination of the sand and the minuteness of the lamination, and the grouping of the layers into conical mounds, are against the notion of a violent flood of water, but rather indicate a slow and long-continued process, in which currents from different quarters, and laden with various kinds of detritus, took a large share. Two currents coming from opposite directions and each charged with sediment, would deposit a part of their burden along the line of their junction. But it is often difficult to understand from the existing form of the ground where the kames occur, how two such currents could have been produced, and how, above all, they should have dropped their loads in such places as we now find the kames to occupy.

The kames, whether of unstratified shingle, or of well-bedded gravel and sand, merge imperceptibly into a wide expansion of undulating ground, the uneven surface of which results from the uneven deposition of the same materials of which the more marked ridges or kames are formed. It is, in short, a tract of gravelly and sandy detritus, which is in some parts heaped up into long, well-defined mounds.[9]

[9] Trans. Geol. Soc. of Glasgow, 1, part ii. 114-117.

Mr. J. Geikie [1] tells us " The kames sometimes run for many miles like great artificial ramparts, some of the more conspicuous ones being upwards of 400 or 500 feet broad at the base, and sloping upwards at an angle of 25° or even 35° to a height of 60 feet and more above the ground. More commonly, however, they occur as mounds and ridges confusedly intermingled, crossing and recrossing each other at all angles, so as to enclose deep hollows and pits between. Seen from some dominant point such an assemblage of kames looks like a tumbled sea. The ground, now swelling into long undulations, now rising suddenly into beautiful peaks and cones, and anon curving up in sharp ridges, that often wheel suddenly round, so as to enclose a lakelet of bright clear water. . . ."

The kames are largely developed in Scotland. " Fine examples," says Mr. Geikie, " are seen in Lanarkshire, in Carstairs and Carnwath. They are also well developed at Haddingtonshire near Cockburnspath ; in Berwickshire at Dunso, and north of Greenlaw ; in Roxburgshire at Eckford ; and another fine set is seen in the valley of the Tweed ; at Wark and Cornhill, Northumberland. At Leslie and Mark Inch in Fifeshire a similar series occurs. . . . Considerable accumulations extend along the low grounds of Kinrosshire, opposite the valleys that descend from the Ochils. The Leven, shortly after leaving the loch, passes through a great series of mounds. Mounds, irregular banks and extensive sheets of like materials, attain a vast development along the great trough lying between the Perthshire Highlands and the Ochils. In Forfarshire, and the whole north-east of Scotland, the greatest gatherings of sand and gravel occur in similar positions. Vast deposits crowd the valleys of the Don, the Dee and the Ythan, and similar appearances are repeated in the rivers that drain north-east into the Moray Frith, as the Deveron, the Spey and the Findhorn. In the neighbourhood of Inverness like masses of water-worn materials form conspicuous objects in the vale of the Ness, and at the head of the Beauly Frith. [2] The most remarkable assemblages of these mounds I have myself seen, are at the mouth of the Sutherlandshire valleys, and notably of the Brora.

[1] Great Ice Age, 228-9.　　　　　　　　[2] Id. 229 and 238.

In different parts of the coast of Aberdeenshire vast heaps of gravel and sand are found, apparently overlying the clays. They occur in mounds and long straggling ridges, like the kames just described. They generally rise from 30 to 40 feet from their base. They have steep sides, and their crest is so narrow that two carts could hardly pass each other on them, and their breadth is such that a stone could be easily thrown right over them. They consist of sand, gravel and water-worn pebbles, sometimes very coarse and without any arrangement, sometimes finer and passing into undulating sandy layers. Broken shells, such as the massive hinge of the Cyprina Islandica, occur both in the coarse and fine varieties. In some localities the gravel ridges are dotted over with large erratics, either rounded or rugged, with few sharp angles.

In regard to their vertical distribution they occur from the sea level up to 1100 feet and upwards. Mr. J. L. Jack has detected them in the Fintry Hills at a height of 1280 feet above the sea.[3]

Kames occur in England as well as in Scotland. Thus in Northumberland we have the so-called Bradford kames on the road from Lucker to Bamburgh. Many kames occur in Cumberland.[4] Similar mounds occur on the western slopes of the Lancashire hills and in the Lancashire valleys; others in Cheshire, as in Delamere forest; at New-church Common and Ellesmere and in North Wales, between Minera and Llangollen valleys.[5] They have also been noticed in some of the Yorkshire valleys,[6] and they have been described from Blakeney in Norfolk and near Great Massingham.[7]

As to the origin of the kames there is now virtually one opinion only. Dr. Buckland, in the "Early Days of Glacial Geology," described some of them as moraines, but the rounded pebbles of which they consist, the occurrence of shells on some of them, and the stratification of others, are all con-clusive against their being the products of glaciers. As Mr. Geikie says : " The generally undisturbed appearance of the bedding shows that the mounds have not been caused by glaciers advancing upon and pushing up before them pre-existing

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[3] Id. 252.   [4] See Geol. Mag. 1879, iii. and 1883, p. 438.
[5] H. B. Woodward, Geology, 490.
[6] Geol. Mag. 1879, 382.   [7] Id. 510.

beds of sand and gravel." [8] Their aqueous origin was noticed long ago. Thus it was hinted at by Lyell, who, in surrendering his views about drifting ice in favour of glaciers, confessed that there are facts connected with the ridges of stratified materials resting upon till which he was unable to explain. [9] Again, in describing the great transverse barrier at Glenairn, in the valley of the South Esk, which resembles an artificial dam, 200 feet high, with numerous hillocks on the surface, he says, " The lower part, 30 feet in depth, laid open in the river cliff, consists of impervious, unstratified sand full of boulders . . . . the upper part of the barrier is composed of from 50 to 100 feet of very fine stratified materials." While identifying this barrier with a moraine, Lyell says " that the *accumulation of so great a capping of stratified materials is the most obscure character of the deposits under consideration.*" [1]

Again he says: " At Cortachie, about four miles below the barrier of Glenairn, the South Esk enters the country of old red sandstone, and a mile and a half lower it is joined by the Proson, and a mile yet lower by the Carity. In the district in which these streams unite there is a considerable thickness of unstratified matter full of Grampian boulders *and covered for the greater part with stratified gravel and sand. In some cases the latter* exhibit the diagonal laminæ common in subaqueous formations; and in others the strata are so contorted that a perpendicular shaft might intersect the same bed three times. In the latter instance the surface of the subjacent red boulder clay has not partaken of the movement by which the stratified deposit was contorted. . . . In the middle of the tract between the South Esk and the Proson is a dry valley, and to the south of this valley, near the Proson, an excavation was made ten years ago, which exposed, extremely contorted, beds overtopped by others perfectly horizontal, having been formed by tranquil deposition after the disturbance of the strata previously deposited," and Lyell confessed that it was difficult to explain the accumulation of the stratified materials overlying the lill, the top of which materials must be 600 feet above the level of the sea, and facing the Strath. [2]

Again he says, " The chief objections (to the glacier theory) are the stratification of the upper part of the banks, and the

[8] Great Ice Age, 247.   [9] Id. 338.   [1] Id. 340.   [2] Id. 341.

necessity of assuming a glacier 34 miles in length, with a fall of only 300 or 400 feet of country." [3]

Other writers have almost universally attributed the kames to water acting in various ways.

"Among the peculiarities of Holderness," says Phillips, "may be reckoned the irregular mounds of gravel and sand (containing crag shells, near Heddon and Paghill), which denote the ancient action of sea currents, for all Holderness was a sea-bed in 'the glacial' period. Of these the long curved mound of Brandsbarton, which in Norway would be called an 'as' and in Ireland an 'escar,' is one of the most singular. It has yielded remains of the mammoth. Skipsea Brough is probably an 'escar' which has been scarped and moulded by Norman, or earlier than Norman art, just as a gravel heap at Bainbridge, in Wensleydale, has been squared to form a Roman camp. Hamilton Brough may belong to the same class." [4]

Mr. Horne wrote in the report of the B. A. for 1861, p. 115, an account of the kames at Bedshiel, near Greenlaw, and argued that they had been formed of marine shingle when the land was at a lower level. Mr. G. Tate, F.G.S., attributed them to the action of water at a remote period. [5]

Professor Geikie says, "The gravel stones, of which the kames at lower levels are largely composed, are almost invariably well-rounded, and water-worn. . . . The chief kame deposits occur in valleys at or near where the rivers escape from the confined mountain glens to enter upon the broad lowlands, and their development is often proportional to the extent of the drainage area. They nowhere occur in the undulating moorlands of Lewis or the great flats of Caithness. . . . . The conclusion seems forced upon us that all these accumulations of water-worn materials owe their origin to currents that once flowed down the valleys."

While attributing the formation of some of the kames to fluvial action at a time when the rivers were of portentous size, Geikie attributes others to the agency of the sea. "The diagonal bedding they sometimes display," he says, "points to the action of shifting currents in a somewhat shallow

[3] Id. 343.   [4] Rivers, Mountains, and Sea-coast of Yorkshire, 124.
[5] Geol. Mag. i. 45-6.

sea. . . . Those who have moved much among the western
islands will, no doubt, have a vivid recollection of the strong
and rapid tides that sweep to and fro along the narrow straits.
With such currents flowing through the submerged valleys of
Peebleshire, ancient river gravels, moraines and till would be
eroded, redistributed, and heaped up into banks and ridges.[6]

Professor Green has shown how in the case of many of
the kames their shape is due to the mode in which their
materials have accumulated. Thus he says, "It not unfre-
quently happens that the long ridges run together and enclose
oval-shaped hollows *without an outlet* which are sometimes
still occupied by tarns. These hollows cannot have been cut
out of a sheet of gravel by rain or river action, because there
is no road by which the stream could escape, and the mounds
of gravel must have been piled up in heaps around the central
hollow. When cut across again the gravel of the kames is
seen to be distinctly though irregularly bedded, and the beds
arch over, so that in a general way the direction and amount
of the dip is about the same as the slopes of the surface of the
ridge. This," he says, "is just the structure that would be
produced if the materials had been heaped up by currents
coming alternately from opposite quarters. . . . At very many
spots where eskers occur, exactly such conditions as these
would be produced if the land were submerged. Eskers are
extremely common, for instance, where large mountain
valleys open out into flatter country. Supposing the sea to
encroach as far as the mouths of the valleys, the load of *débris*
brought down by the mountain torrents would be tossed about
alternately by the stream and the incoming tide, and arranged
in mounds and ridges. . . . Another possible locality for
eskers is a valley which submergence would convert into a
narrow strait connecting opposite seas. Along such a passage
tides coming in opposite directions race furiously, and when
they meet the materials swept along by the currents are piled
up in mounds and ridges having the outline and structure of
eskers. Some fine groups of eskers are perched on plateaux;
in such a case we find that a certain submergence would con-
vert the plateau into a low spit of land over which the tides
would wash at high water from opposite quarters."[7]

[6] Great Ice Age, 228-251.   [7] Green, Geology, 632-3.

Kames are called eskers in Ireland.

Mr. H. W. Jenkins, writing in the *Geological Magazine*, says, " In case any of our readers may ask the question, What is an esker ? we may depict it as a ridge, or, rarely, a mound of sand or gravel, heaped up by the action of water, and derived from masses of the same material in close proximity to it. These masses, though they occur elsewhere, are most abundant in Ireland, where they have received the name of ' eskers '; they are analogous to the sand-banks, harbour-bars, shoals, etc., now in process of formation, through the antagonistic action of tides and currents causing the accumulation of bottom-material at particular points." [8]

Mr. Kinahan, who tells us that in Ireland the esker systems extend sometimes for over a hundred miles, says " that they are well-defined ridges on low ground, breaking into shoal eskers (consisting of irregular mounds and short ridges), crossing high ground, while if a hill occurs they are either deflected or form fringe-eskers round it, or there is a break." He argues that the esker drift is a washed boulder drift or post-drift gravel. Hence why the basal beds which have not been thoroughly washed are often unstratified.[9]

Elsewhere the same writer says, " The formation of the eskers is a disputed subject, but it seems probable that they are modifications of the banks and shoals which accumulated at the colliding and dividing of the ' flow ' tide currents of the esker sea ; similar to those that are found in the seas round Great Britain and Ireland at the present day. In the Irish Sea, in the vicinity of the Isle of Man, there is a meeting of the north and south ' flow ' tide waves, or a ' head of the tide.' Here the tidal currents meet and neutralize one another in one place, forming a mass of currentless water, that simply ' rises ' and ' falls ' and deposits there silt and other materials. The other heads of the tide in these seas are south and east of England, in the Straits of Dover, and between Norfolk and Holland. But in these places there are different results as the currents collide and pass one another, for greater or less distances, and at their edges or the junctions of the different currents long banks of gravel and shingle accumulate. It is also found that long banks of

[8] Geol. Mag. i. 34.   [9] Id. 190.

gravel and and shingle may form at the dividing or splitting up of the 'flow' tide current. This is exemplified off the south-east coast of Ireland. From Greenore Point a main current runs northwards up the Irish Sea, while secondary currents branch off into Wexford Bay; and at the junction of these currents with the main current there are long banks between Greenore and Wicklow Heads. Similar results must have occurred in the esker sea."

The flow tide wave entering at Galway must have sent a main current eastward to the coast near Drogheda and Dublin; south of the current there would be a bay somewhat similarly circumstanced to Wexford Bay, off which banks would form between Galway Bay and the Dublin mountains, that is in the line of country occupied by the principal eskers.

Mr. Kinahan goes on to say very frankly, "The typical eskers are very unlike shoals and other sea banks, being more like railway embankments, some being so narrow that you could nearly shake hands across them, and he argues that it may be as the sea shallowed and the shoals and banks became 'awash,' the current would have the power of changing the massive banks into narrow ridges," and he says "that at the half-tide, or 'awash,' portions of banks and in other shallow places where two currents collide there are esker-like ridges; as St. Patrick's Bridge between Kilmore and the Saltees, county of Wexford, and on the Doggerbank off the mouth of Wexford Harbour. . . . No action but marine at the present day forms ridges at all like the eskers. . . . In no esker will you find any fragments that have not some marks of water action." [1]

Similar to the kames and eskers, but on a much larger scale, are the äsar or osar.

Mr. J. Geikie thus speaks of them: "Resting sometimes on till, but oftener perhaps upon the solid rock, appear certain great natural embankments, or long winding ridges, which are known in Sweden as äsar, and in Norway as raer. They generally arise abruptly to a height that may vary from 50 to 100 feet above the average surface of the ground. Sometimes, however, they reach as much as 180 feet, while now and again they sink to 30 or 20 feet, or even disappear

[1] Geology of Ireland, 225-230.

altogether below newer deposits. Their sides have an inclination of from 15° to 20°, but occasionally as much as 25° or 308, and the two declivities very seldom slope at the same angle. Often beginning in the interior of the country, the äsar follow the valleys down to the low coast land, across which they pass as well-defined ridges out to sea, after a course of not unfrequently more than a hundred English miles. At greater heights than 300 feet above the sea these remarkable ridges are, as a general rule, confined to the valleys, but at lower levels they seem to be tolerably independent of the present configuration of the ground. They are met with at all levels." According to Erdmann they occur at Herjeädal between 1300 and 1400 feet above the sea, and Törnebohm says that in the mountain valleys of the north they go up to elevations of over 2000 feet.

" The materials of which they are composed may consist either of coarse shingle, or of pebbly gravel, or of sand, or they may be made up of all three. In some parts of a ridge shingle and gravel predominate, in others sand is the principal ingredient. In one place the stratification will be distinct, in other places obscure, and not infrequently false bedding appears. . . . Erratic blocks are frequently found perched upon the top of an äs, or plentifully sprinkled along its sides and sometimes they also occur in the interior, especially towards the top or at the base.

" A fine glacial clay containing arctic shells often covers the slopes of the äsar." [2]

Whatever theory we may adopt to account for the äsar, we must, it seems to me, most unmistakably have recourse to water if we are to explain their water-worn pebbles, their stratified structure, their curious outlines, and the shells they contain; no theory of land ice is competent to explain them.

Murchison, quoting Nordenskiold, refers to the gravel, sand ridges and pebble beds, which abound in Finland, and which are, in fact, nothing different from the äsar of the Swedish geologists, and thus the drift phenomena on either side of the Gulf of Bothnia are shown to be identical subaqueous deposits. . . . neither in the south of Sweden, nor in Finland

[2] Geikie, Great Ice Sheet, 385-387.

are there any moraines, all the detritus around the great erratics being water-worn." [3]

Crossing the Atlantic, the Americans call the long mounds, apparently like the Scandinavian äsar, "horsebacks." "The name," says Agassiz, "is somewhat descriptive, for they are shaped not unlike saddles with sloping sides and flattened summits. They consist of loose materials of various sizes, usually without marked evidence of a regular internal arrangement, though occasionally traces of imperfect stratification are perceptible : sometimes they follow horizontally, though not with an absolutely even level, the trend of a rocky ledge. Again they themselves seem to have built the foundation of their own superstructure, being composed of the same homogeneous elements which cover the extensive flats over which they run with as great regularity as upon a more solid basis. The longest of these horsebacks—and they sometimes stretch, as I have said, for many miles—trend mainly from north to south, though their course is somewhat winding, seldom following a perfectly straight line. They are unquestionably of a morainic nature, and yet they are not moraines in the ordinary sense of the term, but rather ridges of glacial drift heaped up in this singular form, as if they had been crowded together by some lateral pressure. Had they been accumulated and carried along upon the edge of the glacier, they could not be found in their present position. They differ also from moraines proper, in their rounded materials, containing many scratched and polished pebbles, while moraines are built chiefly of angular fragments of rock." [4]

"The stratified gravels," says Sir William Dawson, "do not, like the older drift, form a continuous sheet spreading over the surface. They occur in mounds and long ridges, sometimes extending for miles over the country. One of the most remarkable of these ridges is ' the Boar's Back,' which runs along the west side of the Hebert River in Cumberland. It is a narrow ridge, perhaps from ten to twenty feet high, and is cut across in several places by the channels of small brooks. The ground on either side appears low and flat. For eight miles it forms a natural road, running north and south. It

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[3] Murchison, J. R. G. S. xxxiv. clxxvi.
Geol. Shetches, 2nd ser. 111-112.

appears to extend from the base of the Cobequid Mountains to a ridge of sandstone that crosses the lower part of the Hebert River, and consists of gravel and sand. Another very singular ridge is that running along the west side of the Clyde River in Shelburne county. It is higher than that on Hebert River, but like it extends parallel to the river. There are good samples of the hogs'-backs, or horses'-backs, and gravel ridges occurring in many other places, and sometimes accompanied by circular and oval mounds as regular as if thrown up artificially." [5] Dawson attributes them, to the action of water while the land was being elevated. "Many of the mounds," he says, "have evidently been formed by currents of water rushing through and scooping out the present valleys. Some of the more regular ridges are apparently of the nature of the gravel beaches which are thrown by the sea across the mouths of bays and coves." [6]

In addition to the other arguments above used for their aqueous origin, may be mentioned the fact that mounds similar to the hogs'-backs have been noticed in the Peruvian plains, when no ice action can be postulated, and where they must have resulted from the action of water.

What is true of the kames, eskers, etc., etc., is true, it seems to me, of the phenomenon known as crag and tail, which, as was pointed out long ago, is consistent only with water running past some obstacle and depositing a tail of gravel and mud in front of a rock or other opposing mass, and seems quite inconsistent with the action of ice, which would scour the whole surface clean.

Having discussed the arrangement and structure of the *stratified* drift beds, let us now shortly note their wide geographical distribution.

Murchison, in the " Geology of Russia," says, " In ascending the banks of the Dwina from Archangel, it was our good fortune to observe certain beds, which from their structure, fine lamination and thickness, indicated the action of slow and long-continued deposit, while the shells abounding in them (almost undistinguishable from those of the adjacent White Sea) announced that this tract was submerged during a very recent period. We first noticed these beds on the

[5] Dawson Acadian Geology, 82 and 83.   [6] Id. 83.

right bank of the Dwina, opposite the post station of Schastoz-erskaga, about 240 versts above the city of Archangel. At this spot we were much surprised to find a profusion of shells, having a very modern aspect, regularly embedded in clay and sand of about ten feet in thickness . . . . We afterwards found similar beds on the right bank of the Vaga, two stations further to the south. . . . Count Keyserling. . . . has discovered that in the sections on the river banks of the upper portion of the Vaga, such shelly beds are not visible. We might, therefore, view them as relics of the sea, when the present embouchure of the Dwina was a bay or estuary which entered for upwards of 250 versts southward into the adjacent lands. At the same time it is necessary to remember, that these shelly beds are covered by sand and gravel, which we should have great difficulty in separating from the superficial northern drift. . . . there can, we presume, exist no doubt that our deposits of the Dwina and the Vaga are analogous to those described by numerous authors as forming raised beaches (Post Pliocene) at different levels in the British Isles, Sweden, Norway, etc. . . . the coarse gravel and sand which surmount the beds of shells upon the Vaga and Dwina, and which we cannot well separate from the erratic block pheno-menon, must all have been accumulated under water." [7]

Let us now turn to Scandinavia. M. D'Archiac long ago said of these beds, " Les dépôts sableux, avec ou sans blocs, des parties élevées de la Suede, lesquels atteignent jusqu'a 1000 mètres d'altitude et sont horizontaux sur de très grandes étendues, de même que l'etat et la disposition de leurs élements, ne peuvent être attribués qu'a des phénomènes aqueux." [8]

In Norway, Kjerulf describes " beds with Arctic shells as occurring at a height of 1100 to 1160 feet above the sea, while shell-beds of the same age go up to 530 feet. He allows forty fathoms (240-250 feet) as the depth at which the high level beds were accumulated, which added to 400-460 feet will give us 660-700 feet. The shell banks would require some ninety feet of water for their formation, and this, added to 530 feet, gives 620 feet." [9] These high level shell deposits only occur in the south of Norway, and not in the north.

[7] Op. cit. i. 327-332.   [8] D'Archiac, Hist. des Progrès, ii. 53-54.
[9] Geikie, Prehistoric Europe, 271.

In Erdmann's " Exposé des Formations quaternaires de la
Suede," he has much much to say about the stratified gravels
and clays which are so widely spread in Scandinavia, and which
he attributes to the action of the sea. Beds of stratified clay
and sand occur on the northern flank of the plateau of Smälan-
dais-Vestregoth, at 700 and 800 feet above the sea-level. In
southern Halland and on the borders of Smäland and Scania,
they do not occur higher than 200 feet, while in Scania
itself they are only found between 50 and 100 feet. In
Jemteland, in the neighbourhood of Ostersund, the stratified
clay occurs at a height of 1000 feet." [1] These clays are in
some cases found covering masses of angular gravel, and in
others resting directly on the rock, and Erdmann says of
them : " La stratification réguliere qui charactérise ces
couches, et la conformité générale qu'elles présentent entre
elles dans leurs charactères physiques et stratigraphiques,
fortement accusés dans les localités mêmes les plus éloignées
les unes des autres, tout cela montre, à ne pouvoir q'y
méprendre, qu'elles ont été déposées dans une mer trés vaste." [2]
Elsewhere the same writer describes these clays as distributed
over a very wide area in Sweden, from Scania as far as
Norrbothnia, with the same physical and stratified character,
and he argues that over this wide area they must have resulted
from the operation of the same causes. He argues that they
were probably deposited in an Arctic sea from the occurrence in
them of shells of the species Yoldia, Saxicava, Leda, Cyprina,
Arca, Natica, Astarte, etc. Where the shells fail, which is the
case in most places, this deposit is easily distinguished by its
foliated and stratified character. Our author describes it
thus : " Partout elle consiste en une suite de couches alternant
entre elles, ordinairement très minces, parfois aussi d'un pied
et plus d'epaisseur ; d'une contour tantôt plus ou moins
claire, tantôt plus ou moins poucée. Une coupe transversale de
l'assise entière présente de la sorte une série de feuillets ou
de strata differemment colorés si nettement determinée,
que si même comme c'est ordinairement le cas, les débris
coquilliers y font defaut, dès qu'il est question de déterminer
l'age relatif d'une de ces assises, il est impossible de se

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[1] Op. cit. 25, note 1. [2] Id. 25 and 26.

méprendre à l'egard de la place qui leur appartient dans la série des formations quaternaires." [3]

Erdmann tells us that as we leave the lowlands bordering on the Baltic and the North Sea, and advance into the higher country, these clays become less plastic, and more mixed with sand, and eventually pass insensibly into gravel and sand. The height at which they occur is very variable. In Sweden he supposes the glacial sea covered all the land south of 61°, except a plateau extending from Falbygd in Westrogothia, as far as Scania and Bleking. He makes the stratified sands and gravels occurring at higher levels the equivalent of the clays already described. They abound in the government of Jönköping, the north-east of Dalecarlia, Jemteland, Lapland, etc., and he says that, without doubt, like the clays, they have been subject to the action of the waves. In some places these sands alternate with the stratified clays, as in the valleys of the lake of Asunden along the river Atra, etc., etc.,[4] on the sea-board. Especially on the western coast, and up to a level of 500 feet there occur, but always near the sea, banks of Arctic shells which seem to be absolutely contemporary with these beds.

It will be seen, therefore, that in Scandinavia we have ample and widespread evidences in the existence of the stratified sands and clays of aqueous action having succeeded the period of the great glaciers. I shall revert to evidences of another kind in the same area pointing to the same conclusion presently.

Murchison, describing the drift of Denmark, says "the blocks are associated with sands and sandy clay containing fragments of modern shells like those of the adjacent open northern sea, viz., *Balanus, Corbula nucleus* and *Cyprina Islandica*. . . The blocks often occur in lines along great fractures or parallel to them, and are then associated with *Mytilus edulis, Cardium edule, Buccinum reticulatium*, all indicating raised beaches. Again, wherever blue clay prevails, the *Mya* or *Hiatella Arctica* occurs, which being a deep sea shell, indicates that it has been thrown up and mixed with littoral shells." Murchison then goes on to affirm his agreement with Forchammer, that the

[3] Id. 68-69.   [4] Id. 77.

northern detritus of Denmark must have been accumulated under water.[5]

The drift accumulations of North Germany have been held by many geologists to have been deposited on a sea bottom, at a time when all the low ground of Denmark, Holland, North Germany, Poland, and a wide region in middle Russia, were submerged. . . . The occurrence of marine shells here and there in the boulder clay of Prussia, seems to afford strong confirmation of this view. . . . Berendt has speculated about the former existence of a widespread " diluvial sea." . . . We have certain proof that the land has been submerged in North Germany. Between the upper and lower clays of the province of Prussia come beds of sand and gravel, which, according to Berendt, have yielded a number of shells of marine mollusca, such as Cardium edule, Nassa reticulata, Cyprina islandica, Mactra solida, and from the same beds Jentsch has recorded Yoldia (Leda) Arctica, and the fresh or brackish water form Paludina diluviana. . . . In these beds we have, according to Penck, a marine littoral accumulation ; they mark the shores of an interglacial sea, upon which the shells were cast up and rolled about by the waves.[6]

As we proceed westwards, the coarser deposits give place to great beds of sandy drift. The best known of these are the so-called Campinian sands of Belgium, so well described by Devalque, who treats them as the western prolongation of the great sheet of sand which stretches through Holland, where it is known as the Diluvialzand, and which extends also into Northern Germany along the borders of the Baltic.

Agassiz mentions stratified beds of gravel on the Jura, which he says belong to the same era as the dispersal of the erratics. These beds consist of rounded pebbles and of more or less fine sand and loam, irregularly stratified and false bedded. These beds, he says, were formed in the same way as the stratified moraines to which he had already called attention.[7]

The most cursory outlook on the part of any traveller approaching the Alps, discloses in almost all the lower country continuous sheets of deep water-worn gravel spread over the

[5] Russia and the Ural Mountains, i. 541-2.
[6] Geikie, 277-281. [7] Id. 283-289.

whole country, regardless of the contour, and apparently spread out by water and nothing else. These deposits are especially remarkable in the wider valleys, as those of the Upper Rhine, the Aar, and the Rhone, and this view is universally maintained, so far as I know, by the French geologists.[8]

Similar beds of stratified drift occur in the British Isles at considerable elevations, generally arranged in terraces on the slopes of hills. Professor J. Geikie has described the shelves of gravel at Eaglesham in Scotland as reaching up to 800 feet above the sea-level; and he tells us how he had met with similar terraces at a greater elevation. Thus he had traced them on the Moorfoots up to a height of 1050 or 1100 feet. Still further south similar beds had been noticed by Mr. H. M. Skae in Nithsdale, at a height of 1250 feet above the sea, and he tells us that several instances of the occurrence of stratified accumulations of sand, gravel, clay, and silt occurring at considerable elevations in the Northern Highlands are given by Mr. Jamieson.[9]

In Scotland the drift with marine shells has occurred at Chapelhall, near Ardrie, 510 feet above the sea, at Fauvre in Banff 350 feet, in Caithness and Aberdeenshire 250 feet. Near Bothwell at 200 feet, in other parts of the Clyde valley and elsewhere at various lower levels down to the 40 feet terrace. The highest of these beds at Airdrie was only a limited patch some two feet thick, embedded in boulder clay, and may have been transported; but giving the deposit its full value Mr. Bell concludes that it points to a submergence of 500 feet in Scotland.

Speaking of Aberdeenshire, Jamieson describes a widespread accumulation of water-rolled gravel often of great thickness and quite destitute of fossils. It is poured out in greatest profusion towards the mouths of valleys, and pervades all the river basins he had examined, decreasing in extent in ascending their course. It also covers many tracts where no river appears to have existed. All along the Dee from Aberdeen to Braemar, nearly sixty miles in the interior, this upper rolled gravel is everywhere found. At Aberdeen it

[8] See Falsan, La Periode Glacier, 126, etc., etc.
[9] See Q. J. G. S. 1865, p. 177.

forms swelling mounds and little hills, on which a large part of the city and its suburbs is built. In many places it is spread out in wide horizontal sheets, as at Aboyne, the Moor of Dinnot, Ballater, and elsewhere. Again, he says it is met with tumbled up in tumultuous hillocks or longtitudinal mounds parallel to the strike of the river. It is always distinguishable from the subjacent drift (1) by the absence of the striæ or glacial *burinage* on the pebbles; (2) by the highly water-rolled aspect of the deposit; (3) by its texture and different hue.[1]

This water-rolled gravel he had noticed along the Don, the Ythan, the Deveron, the Findhorn, the Spey, the Tummel, the Tay, and other streams, and it everywhere presented similar features, and looked as if formed out of the pre-existing drift, being merely the stones and sand of that deposit, the clay and muddy matter having been washed out. It fringes the sides of many of the larger valleys to heights sometimes 200 feet above the adjoining stream, and is displayed in many places out of the way of all rivers, and is frequently accumulated in long mounds behind masses of rock that present bare and craggy faces on the opposite side.[2]

Jamieson remarks that the side glens frequently contain a great thickness of drift, generally of a looser and sandier character than that of the main valley, and the same is true of the higher part of the Dee valley, especially is this the case in the centre of the valley. In Glen Carch, on the east of the mountain of Ben a Buird, this deposit consisted almost entirely of stratified sand and gravel, evidently well washed, and deposited by water. Imbedded in it were many masses of red granite from the neighbouring mountains, some of which were from seven to fourteen feet in diameter. Some were rounded and some angular, scratched boulders were rare. Much of the deposit containing these blocks consists of pure sand, often laminated. It was nearly 2000 feet above the sea level. . . . In the valley of Loch Ehchan, at about 2400 feet above the sea, there is a remarkable assemblage of mounds and tumuli, some of them rising to a height of 100 to 150 feet above their base. The gravelly *debris* on their steep slopes contains many fragments of gneiss and laminated

[1] Q. J. G. S. xvi. 349.   [2] Id. 350.

quartz, and some of a hornblendic character. These seem to be foreign to the valley, and Jamieson says: "that if this be so it tells strongly against their having been glacier-borne to their present position. Many of the quartz fragments are partially water-worn." [3]

. . . A section at the base of one of these mounds consists of the finest laminated silt, the laminæ being partly of fine sand and partly of clay, capped by a bed of stratified sand with boulders interspersed.

Turning to England, I shall next make some extracts from my friend Mr. H. B. Woodward's admirable manual.

In Northumberland we have great stretches of pebbly gravel, described by Mr. Lebour as moor gravel. This is apparently of the same age as the mounds known as the Bradford kames, on the road from Lucker to Bamburgh. Mr. J. Clifton Ward has described the stratified sands and gravels (which often occur in the form of eskers) occurring in Cumberland, which he attributes to marine action and which are no doubt the reassorted materials of the lower drifts. In Lancashire and Cheshire the similar beds also contain marine shells, while the boulder clay is occasionally partially stratified. In the Isle of Man, and notably in the cliffs stretching from Ramsay to the Point of Ayr, there is a great series of sands, gravels, and shelly clays, containing marine shells. Professor Hughes has done a good deal to emphasize the distinction between the true glacial deposits in North Wales and the subsequently distributed drifts, which, according to him, were spread out by marine currents. The former are the products of local glaciers, the latter consist largely of transported materials. The marine origin of these later drifts of North Wales is attested, according to Mr. Strahan, by their well-marked stratification as a whole, by the alternations of sands and gravels with the boulder-clay, and by the occurrence through all the beds of marine shells.

From the occurrence of marine shells in the sands and gravels of Shropshire, Staffordshire and Worcestershire, Murchison and Professor Buckman were led to infer an "Ancient Strait of Malvern," and that the sea had covered the valley of the Severn from Bridgnorth to the Bristol

[3] Q. J. G. S. xvi. 356-7.

Channel. It is quite likely that this was the case, but a large area of the midland and eastern counties, says Mr. Woodward, was at the same time submerged." In the Cotteswolds and neighbouring valleys are stretches of drift gravel containing pebbles, probably derived from the boulder clays of the North of England, which, according to Mr. Lucy, are certainly not glacial gravels and can only be attributed to aqueous depositions.[4]

" The chalky boulder clay of the Trent basin," says Mr. H. B. Woodward, "in places presents signs of aqueous origin. The chalky gravel is also a marine deposit." The shelly clays of the east of Yorkshire as at Bridlington, Dimlington, Hessle, etc., point unmistakably to water having influenced their deposition, as do the laminated clays and bedded loams and sands of the same district; the Purple clay is in some places stratified, while the Hessle gravels are distinctly so, and have been generally treated as a marine deposit. In Norfolk the brown till contains fragmentary marine shells, while the contorted drift is conspicuously a water, and not an ice, deposit. Mr. Woodward describes it graphically as consisting of seams of very chalky loam, marl or boulder-clay occurring in irregular and lenticular masses. The drift here in fact becoming a conglomeration of all kinds of sediments, containing masses or nests of sand, with marine shells and sometimes coarse gravel, although loam and marl predominate. In many places the beds are disturbed and twisted into S-shaped contortions. " The contorted drift," says the same author, " which exhibits evidence of stratification and sometimes of ripple marks, was probably deposited in the sea. In regard to the so-called middle sands and gravels containing marine shells, there is a general agreement that they were deposited by water."

Messrs. Wood and Harmer conclude that the shells bear evidence of having been shifted and rolled by currents, which brought them from some other part of the sea bottom, while the more delicate shells may have been transported by seaweed. The plateau, or so-called cannon-shot-gravel, which covers so much of East Anglia, and has sometimes been called flood gravel, is unmistakably of aqueous origin, and, as Mr.

[4] Woodward, Geol. of England and Wales, 487-493.

Woodward urges, points to a diluvial agent as its depositor.[5]

These instances prove how widely spread the aqueous elements of the drift in England are. They have also occurred at considerable elevation.

Mr. T. Millard Reade says of them : " At Moel Tryfain, Carnarvonshire ; at several places in Flintshire (Moch-y-Crio among them) ; between Mimera and Llangollen, Denbighshire ; at Gloppa, near Oswestry ; at the Setter's Dog, near Macclesfield, and the Three Rock Mountain, near Dublin, sands and gravels are found varying from about 1000 to 1400 feet above the sea level. These sands and gravels contain shells of mollusca, speaking generally of a similar *facies* to those fragments found in the low-level boulder clay and sands. . . . Drift with shells is met with at various places at levels intermediate between the low-level boulder clay and sands, and the high-level sands and gravels. This drift is usually laminated and current-bedded. . . In one place (at Moel Tryfain) a laminated bed of the sands inosculated with the till, which also contained pockets of the sand. . . . The stones contained in the sands and gravels are much water-worn. . . . The grains of sand are also much rounded, water-worn and polished." [6]

Darwin tells us that while he had not been able to find shells at Moel Tryfain, he found beds at least twenty feet in thickness of irregularly stratified gravel and boulders, with distinct and quite defined layers of coarse yellow sand, and others of a fine argillaceous nature, and reddish colour. " These beds," he says, " closely resemble those of Shropshire and Staffordshire, in which are found numerous fragments of sea-shells, which everyone, I believe, admits are of submarine origin. It may therefore be concluded that the layers of coarse and argillaceous sand and of gravel, with far transported pebbles and boulders, were deposited when the summit of Moel Tryfain stood submerged beneath the sea." He goes on to suggest that the boulders were carried by icebergs floating in this sea.[7]

In Central England and the country north of the Thames, and the Bristol Channel, marine shells of the glacial period

[5] Id. 493-510.   [6] Geol. Mag. 3rd ser. xix 312.   [7] Id. 358-9.

sometimes reach as high as 600 or 700 feet.[s] Dr. Crosskey argued, from the same beds, that the submergence of the English midlands had been 500 feet at least, and perhaps more.

Professor Jukes speaks of the Irish so-called marine drifts of the low country as largely covered by a widely-spread mass of drift, consisting of dark sandy boulder clay, with pebbles and blocks and occasional beds of sand and gravel, sometimes very regularly stratified. From the predominance of limestone in the gravel pebbles has arisen the name of limestone gravel. It is found up to heights of 1200 feet on the granite mountains south of Dublin. The Curragh of Kildare owes its swelling outlines to a great mass of the gravel from 100 to 150 feet thick. It occurs in hollows on the top of the hills of Castlecorner to a height of 700 and even 1000 feet above the sea, while other spaces at lower levels are quite free from it. It is found often with large blocks high up on the northern flanks of all the hills of the south of Ireland, such as the Knockmealdon, the Gattees, the Slieve Bloom, the Keeper group and the Slieve Bernagh and Slieve Boughta hills; "one long escarpment of drift in Glenbarrow, where the river is at a height of about 800 feet above the sea, and three or four miles from the limestone plain shows cliffs of drift 120 feet high, all regularly stratified."

Sea shells are found in the limestone gravel in Glenismaule, near Dublin, and also in the Dargle valley, and in the valley west of the Sugar Loaf and south of Ennis Kerry, County Wicklow, up to heights ranging from 500 or 600 up to 1000 and 1200 feet. They are found in greater abundance and much better preservation in the sands and marls which overlie or form the upper part of the limestone gravel through the lower parts of the county of Wexford. They are also to be found in the gravels of the more central parts of Ireland, as at Ballymore Eustace in Kildare. "That the superficial deposits now described," says Professor Jukes in words adopted by Sir A Geikie, "were formed under the sea, notwithstanding the absence of sea shells from the greater part of them, and especially their upper part, I have not the

[s] Lyell, Antiquity of Man, 3rd ed. 274.

slightest doubt. A conclusion in accordance with those of Professor Ramsay regarding the drift of North Wales." [9]

Mr. Kinahan speaks of the drift in question as a glacialoid drift. He describes it as always having a distinct stratification, the materials of which it is composed having been arranged by and deposited in water. "It seems," he says, " to be marine on account of its extending over wide areas, and is usually made up of the mixed *débris* of the boulder clay and moraine drift, but in some cases, as in south-west Mayo, it is divided into two distinct members; the lower drift being the *débris* of the boulder clay, while above, over a denuded surface of the first, and often having a parting of thin beds of fine sand or brick clay between, is a drift made up of the *débris* of the moraine drift." He divides the marine drifts into several classes, according to the height at which they occur, and speaks of the highest, which, he argues, was deposited when the sea reached 300 feet higher than now, as the Esker sea drift. " The margin of this Esker sea," he says, " will be found in places in the hills sometimes as a shelf cut in their sides, at other times as a beach accumulation. It is extremely well marked in parts of Tyrone and Derry, forming circles of gravel and sand round the isolated drift hills so common thereabouts. In some places the Esker sea drifts are spread out in undulating sheets, as in the county of Kildare. In places in these sheets there is fine gravel and sand under a shingly gravel. Such sections, however, are only found in narrows, like that in the valley of the Nore, at the city of Kilkenny. This is easily accounted for. When the waters were deep and broad, there were fine deposits accumulating; but as it shallowed and narrowed the current became more rapid and the deposit coarser." [1]

Turning to America, we have first to face the fact of the scarcity of true moraines there.

Dana says emphatically there are no *lateral* moraines of the Glacial era in New England. He also says, that no distinct terminal moraines of the glacial era have been observed in New England, and he accounts for this by the glacier having terminated in the ocean on the east, south-east, and

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[9] Jukes and Geikie, Manual of Geology, ed. 3, 707-710.
[1] Geology of Ireland, 225-6.

south, and by the further fact, that the melting of the Champlain era, that is the era of great floods, took place over too vast an area for the formation of proper terminal moraines, and he argues, that the universal covering of drift on Long Island was deposited during the early Champlain period. In regard to the arrangement of the boulders in many cases in lines or trainées, he says, they are not properly moraines.[2]

The fact is that, in America, as in England, there has been a great deal too much hasty generalization from the occurrence of certain long mounds and ramparts of gravel, etc., to the ready explanation that they are old moraines. On this subject I would quote a few cautious words from Mr. Mallet, who urged that moraines, or masses dropped by ice could not be distinguished generally from torrentially moved masses of clay, gravel, and rock, or from escaks or eddybars, formed by tidal stream action, pointing out two cases, one in Wicklow, the other not far from Dublin, both pronounced by Agassiz to be indubitable moraines, but the former, being manifestly a torrential bank, and the other the effect of a tide stream eddy when the plain of Dublin was still from 500 to 700 feet beneath the sea surface.[3]

While traces of the direct action of glaciers are singularly wanting in many districts of America, those of water action are conspicuous enough.

"I know enough," says Ramsey, a great champion of the glacial age, "of the superficial strata in North America, to foresee that the erratic deposits there will some day also be divided into terrestrial and marine series."[4]

M. Desor, writing in 1850, in answer to those who argued that the drift deposits of the American plains remain now as they were left by the glaciers, urges very strongly that their stratified character in many places points clearly to their having been distributed by water.

Dana speaks of the stratified drift, and objects to the term modified drift, since the materials were often stratified at their first deposition. The stratified drift over the interior of the Continent contains no marine shells, although fresh-water shells are not uncommon. Towards or along the sea shores

[2] Dana, The Glacial and Champlain Eras in New England.
[3] Q. J. G. S., xxix. 224.   [4] Phil. Mag. 4th ser. xxviii. 310.

the stratified beds often contain marine shells. Nearly all the stratified drift, he says, was deposited in the earlier or diluvian stage of the Champlain period.[5]

"The material of the stratified drift was derived by the waters either directly from the melting glacier, or from the loose material remaining on the hills after the ice had disappeared. The beds were deposited either along the valleys and flooded streams, in and about flooded lakes, or in estuaries and along sea borders, making one fluvial, two lacustrine, and three sea border formations."

"The fluvial and lacustrine formations characterize all the river valleys and lake basins, and are of Continental distribution. The fluvial deposit accompanies the whole course of a stream and its tributaries to its sources in the mountains, and fails only when the stream is a steep mountain torrent. . . . In the Champlain period the waters often spread to three or four times the distance of any modern flood, rising to a high level marked off by the upper plain or terrace. These terraces, whether along river valleys or lakes, have generally a flat summit, because levelled off by waters. Commonly there are plains at several levels, in which case the valley is bordered by a series of terraces. Terraces round lakes are significantly called benches. Up and down the streams horizontal lines may be traced for miles, marking the limit of one or more of the several terraces bordering it.[6] The higher flats in the valleys are " the floodgrounds of the river during the great Champlain floods, and the intermediate terrace plains are other levels, formed either during the rise of the flood, the water while on the increase flowing long, it may be, at certain levels. . . . Part may be under water levels, for great streams and lakes or lake borders often have shoals at two or three levels, and part may have been occasioned by the contributions of side valleys."

The stratified beds consist either of clay, earth, sand-gravel or gravel with a large admixture of cobble stones or boulders. Such beds may alternate with each other, but in river valleys clay deposits are more common at the bottom, and pebble and cobble stone beds towards the top. The kind of material is an indication of the rate of flow of the waters during the

[5] Manual of Geol. ed. 1880, 527.   [6] Id. 543-5.

deposition, the bed that consists of clay at one place may thence be of sand a few rods off, or have at times interlaminations of sand, and the high terrace formation, consisting chiefly of sand beds, may be and often is topped by the very coarsest detritus of a river valley, because of the violence of the floods during their later deposition. The clay deposits are most extensive near lakes or where rivers widen out into lakes, and have usually the thin laminations of ordinary river deposits. Logan has called these beds Erie clays, from their great extent north of Lake Erie. These clay beds may rise but a few feet above low water mark in one part of a valley, and a hundred feet or more in another part, this depending on the shorter or longer continuance of the period of quiet waters, and other conditions.

Occasionally such clay beds contain isolated boulders a ton more or less in weight, and are overlaid in places by coarser stratified drift; the stratification varies from the most regular, or that of gently moving waters, to that which would form *only under a vast miscellaneous supply of gravel or sand and water* (the italics are Dana's), generally it is in a kind of flow and plunge style of deposit, due to the successive plunges in rapidly flowing water. Beds of this kind occur with others of horizontal bedding, or sometimes locally in the midst of coarse gravel deposit, such stony gravel not participating in it because of its coarseness.

In many large valleys the formation is the earthy loess, a deposit sometimes of great thickness; it indicates by the absence of bedding that it was made in a prolonged flood, with the waters comparatively quiet and not in violent flow; for the floods of successive years would have left marks of the succession in the bedding; and violent movement would have made oblique lamination.[7]

The so-called Orange sand beds extend all down the Mississippi valley from Kentucky and Missouri to the Gulf, and below Natchez into Alabama and westward into Texas. They consist mainly of sand, but also contain some pebbly beds, and occasionally contain, even in Mississippi, stones of ten to one hundred pounds in weight, and, rarely, of 150 pounds. They also contain some local clayey beds. The stones show the

[7] Id. 546-548.

material came from the north. They have generally the flow and plunge structure. " The facts prove that there was a vast and violent flow of water down the Mississippi valley, bearing an immense amount of coarse detritus.[8] The Orange sand is 40 to 100 feet thick, and in some places over 200. Towards the Gulf it lies at a considerable level below the water level.

In an Artesian well near the Calcasien river (200 miles west of New Orleans), the Orange sand was 173 feet thick beneath 160 feet of clay (Port Hudson group), and at another, 700 yards to the west, 96 feet thick beneath 354 feet of clay.[9]

" About New Haven, Connecticut, there is a good exhibition of the deposits that were made by the flood in an estuary or bay. The beds are for the most part obliquely laminated, and the laminæ *rise to the north*, that is, pitch in the direction of the flow. Further, the effect of plunges in the waters is apparent in the plunge and flow structure of the obliquely laminated beds. Such beds are usually as much as 6 inches thick, but occasionally 6 to 8 feet. A thickness even of 6 inches is proof that vast quantities of sand and gravel were at the disposal of the currents and waves, and that the deposition went on with great rapidity."[1]

The various facts I have mentioned in regard to the materials, the structure and disposition of the drift, will suffice to show what, so far as I know, is not now disputed, that after the great glaciers of the so-called glacial age had done their work, water on a more or less wide scale intervened and rearranged and sorted the materials, and distributed them as we find them. While most if not all geologists are willing to concede so much, they differ materially as to the kind of water which worked the change, and their arguments are in a measure mutually destructive.

A few have ventured to attribute the distribution of the drift to rivers working at other levels and of quite portentous size. In regard to this theory I would quote a few sentences with which I quite agree.

" I have often been curious," says an acute writer, " to know whether the writers who ascribe these diluvial phænomena to the actual operations of atmospheric waters draining off the surface of the earth, ever could have attempted to present to their minds

[8] Id. 548.　　　[9] Id. 549.　　　[1] Id. 551.

anything like a precise view of the districts in question, and of the phænomenon they undertake to explain; for instance, of the structure of Luneburg Heath, and all the vast diluvial flats of the North of Germany. These are occupied by one vast accumulation of gravel, partly chalk flints, derived probably from a zone of that formation, which must originally have occupied this tract (as may be seen from the chalk pit at Luneburgh) but every apparent mass of which has been swept away, and buried beneath its own ruins; but with these are intermingled vast blocks of granite, often as large as small cottages, for which, at a little distance, I have more than once mistaken them; this granite being derived from the Norwegian mountains, on the opposite side of the Baltic. Now I will attempt to explain all this on the fluvial theory. First, the rivers now flowing through the North of Germany must have changed their course so frequently, as to have covered, successively, every inch of the North of Germany, since this gravel is universally distributed, and this they must have done, though we do not find, from the earliest records preserved of the topography of the country, that they are in the habit of changing their course in the least. Secondly, they must have washed away every projecting mass of the chalk formation, although we do not find that the slightest mound of the most ancient entrenchments in the neighbourhood has been sensibly affected by atmospheric causes for some thousands of years. Thirdly, they must have carried blocks of many tons in weight for some hundreds of miles, though they have now unaccountably left off transporting anything of more than a few ounces, and that only for short distances. Fourthly, they must have transported these blocks up their currents; since the granite is derived from the opposite side of the Baltic, but the actual course of all the streams is towards the Baltic." [2]

Again, De Lasterye noticed long ago, that while rounded boulders often occur on the Scandinavian mountains, unrounded ones occur in the river beds, showing that the rounding in these cases could not have been caused by the rivers, and that the rivers could not have transported the boulders very far.

Mr. J. Geikie, who partially favours the fluvial theory, is frank

[2] Phil. Mag. ix. 195, 196.

enough to say that these rivers must have greatly surpassed in volume and breadth their puny representatives. "It is impossible," he says, " to conceive that the masses of gravel and sand occupying the lower reaches of the upland valleys, and some of the Highland glens, could be laid down by rivers like the present, even although these were to continue in constant flood. . . the amount of water circulating in the valleys has in some way vastly diminished." [3]

I don't know on what physical basis such a theory can be made to rest. How are we to postulate the necessary rainfall? where are we to get the necessary drainage area for such mighty rivers within our four seas? Apparently those who favour this view have in their minds the floods which some times follow the melting of the snows in the Alps in the summer, but these floods are strictly limited to the river courses, which become gorged with water, and do not form careering waves of water which could spread great sheets of gravel over the country, irrespective of its drainage. We have already shown the physical impossibility of the theory of great ice-sheets, but even if it were sustainable it is surely inconsistent with the sudden or rapid melting of great masses of ice.

If the glacial climate was so severe that it was possible to accumulate enormous sheets of ice and ice caps, I cannot understand how the summer melting of this ice, on any great scale, can be admitted. Very hot summers in the temperate zone are inconsistent with such a development of the ice as is required for an ice age. If on the other hand the floods were due to the melting of the ice, at the close of the glacial age, the change of climate involved must have been very sudden, or very rapid, much more sudden and rapid than is consistent with any uniformitarian theory.

If there was a slow and gradual amelioration of the climate, there could be no such floods as are required by the theory.

Again, it seems impossible by an appeal to sub-glacial streams to explain the facts as we find them. The water flowing from a glacier either originates along its surface and is due to surface melting, or is due to the melting of the ice-foot by friction. In either case, as we see it in the greatest

[3] Great Ice Age, 243.

glaciers, it runs from underneath the ice in separate streams. These separate streams could not deposit widespread sheets of *débris* in a continuous way, nor can we understand how they could be made to flow at the heights at which the stratified drift occurs. Rivers, unless they are torrential, do not carry along gravel and boulders in the way required, nor do rivers contain marine shells, nor do they spread out great sheets of stratified, or partially stratified, gravels and clays, irrespective of the contour of the country ; nor do they run up and down hill, up to heights of 2000 feet above the sea level, and then deposit marine and not fresh-water shells. Nor can I see how any river could bring chalk flints from Antrim to North Wales, nor Scotch pebbles to Moel Tryfain, nor Scandinavian boulders to East Anglia, nor erratics from Esthonia to Holland. How could the water again pour over hill and dale and leave terraces on the mountain flanks, and stratified beds at heights hundreds of feet above the sea ? If ice can be conceived as travelling up hill, water cannot. Besides, how are we to account for the marine shells, in the stratified beds and in the kames, and for the sifting of the various beds everywhere by such a cause ? Assuredly only the exigencies of a forlorn position has led glacial geologists to appeal to such a cause.

Those who have argued that the äsar, which in some places are 180 feet high, and which run for hundreds of miles in parallel lines, and which frequently have branches, and whose trend is often at right angles to that followed by the boulders, that is, to the general trend of the ice flow, were deposited by sub-glacial rivers, seem to me to have entirely forsaken empirical methods, and to have had recourse to the north wind to shelter them from the wind coming from the east. It makes us rub our eyes to read in the pages of such a cautious and careful writer as Dr. Wright, a sentence like the following, for instance : " New England is gridironed by a system of gravel ridges deposited by glacial streams which were, to a great extent, independent of the minor features in the present topography. In these and in the terminal moraines, we study the skeleton of the continental ice-sheet as intelligently as the anatomist can study the skeleton of a dissected animal." [4]

[4] The Ice Age in America, 314.

Nowhere in the world is it possible, as it seems to me, to find anything in the operations of nature to support such a view. In no sub-glacial streams or rivers known to me are anything like these deposits being formed. Such rivers scour away all the loose sand and mud, and leave piles and trainées of clean stones, but anything like the stratified drift is nowhere to be found among them, nor does it seem to me to be possible to imagine how it could he.

Agassiz says it is easy to prove that the rounded pebbles of Alpine rocks, spread along the whole course of the valley of the Aar, were not transported to their present position by that river, because between the glacier from which it issues and Berne the flowing of the stream is interrupted by the barrier of Kirchet, the lake of Brienz and the lake of Thun ; and because between these lakes its velocity is so small that it transports only mud and very fine gravel, and that the pebbles over which the river flows below Thun do not issue from the lake. " Supposing that the volume of the Aar was formerly greater, why," asks M. Agassiz, "are not the lakes of Brienz and Thun filled in the same manner as the plain of Meiringen and the bottom of the valley which separates the two lakes ? [5]

Speaking of the äsar, Murchison, in arguing against the notion that they have been caused by rivers, says : " However it may be argued that in mountainous tracts torrential rivers and their feeders may have descended as they do now, and may thus have produced rounded materials in valleys, the argument is, at all events, perfectly inapplicable to the formation of the Swedish äsar. These linear ridges have not only been accumulated in long trainées and lengthened mounds or terraces high above the valleys, but offer appearances entirely unlike those produced by rivers.

Professor Ramsay in his " Old Glaciers of North Wales, 101," has drawn attention to a very important fact. Fresh-water streams cannot remove (and it follows that they cannot deposit) drift, consisting of a mixture of huge boulders with finer detritus. They wash away the finer and lighter matter, and leave a concentration of boulders behind. " But it may be remarked," he says, " if fresh water could not have accomplished

[5] Proceedings Geol. Soc. iii. 329.

the clearance of boulder drift so strikingly indicated by the long narrow hollow in Cwm Hafar, how could it have transported the blocks, the abstraction of which has left the magnificent mural cliff at the head of the Cwm ? [6]

Jamieson, who was a hesitating advocate of the fluviable deposition of the gravels, nevertheless says he does not see how any river could so thoroughly scour out the drift as has been done in the narrower parts of most of the valleys and even in their wider portions, leaving in trough-shaped hollows not a trace of shingle or *débris,* and this in localities where the mass of drift must have been immense. Another objection arises from the fact of the drift being piled up occasionally in great undulating mounds and tumuli 40 to 100 feet high, the internal structure of which sometimes shows that their present form is not the result of denudation, in what had formerly been a horizontally arranged deposit, but the undulations conform to the exterior outline. Further, if the excavation of the drift had been owing to fluviable agency, we should expect to find a greater concentration of large boulders along the river channels, and also more evidence of river meadows (or boughs, as they are called in Scotland) at higher elevations than what we see. Then lakes and valleys with chains of lakes and still sheets of water would arrest all the stones and gravel brought down by the stream from the valley above—in short everything except the finer impalpable sand, and when the lake was large and deep, even most of that. The marks of denudation in the neighbourhood of such lakes and rivers are much greater than we should expect on the supposition of mere river action combined with slow emergence. The crag and tail phenomenon is equally inexplicable on such a theory.[7]

Jamieson again urges that in many rivers where the valley narrows and forms a ravine, the sides of the ridges on each side are denuded of drift in a remarkable way, inexplicable by any river action, however produced, for this denudation ascends frequently several hundred feet, sometimes even to a thousand, above the present bed of the stream, and has imparted a bare stony washed aspect not visible on the other sides of the hills, even where the slope was of equal steepness.

[5] Geol. Mag. ix. 23.   [7] Q. J. G. S. xvi. 354-5.

At the pass of Birnam on the Tay, the west side of the Hill of Newtyle has its rocky strata bare all the way to the summit, which is 900 feet higher than the river at that place. The denuding agent has in this case evidently flowed over the crest of the hill, for the rock at the top, which is of coarse clay slate, sticks out in lumpy knobs, the intermediate spaces between which have been swept clean of all small *débris*, while the opposite flank of the Hill of Birnam has been bared of earthy cover to similar heights. The Hill of Craig y bard, north of Dunkeld, is also remarkably denuded even to its summit, which is about 1150 feet high. "There is no polish and no striæ," says Jamieson, " on these rocks showing glacier action as the cause of this remarkable denudation. The long hollow water-shed, stretching from Blair Gowrie to Dunkeld, and terminating at the Loch of the Lorn, which is about 302 feet higher than the river, presents great signs of denudation, consisting of masses of rugged gneiss scoured bare of all drift and *débris*, while the face of the hill to the north has also a very bare, washed appearance.

In America Dr. Wright has remarked upon the extravagant demands on our credulity involved in the fluvial explanation. Thus, speaking of the kames in the Connecticut valley described by Mr. Upham, and which Professor Dana attributes to the action of the Connecticut river, he says : " On this theory, the height of the floods in numerous localities must have been between 200 and 300 feet above low water in the river, for in various places these deposits are of that height above the river. On the supposition that the kames were deposited when the ice itself formed barriers to keep the streams in various abnormal positions, the glacial floods would not need to be more than from 100 to 150 feet in height, since that is all that is required for the deposition of the highest river silt which occurs."

Dr. Wright confesses that Professor Dana's estimates of the size of the Connecticut river floods at that time are somewhat startling, even with all the changes of level for which he provides in his theory. For, after reducing, by reason of the Champlain depression, the gradient of the stream during the close of the ice period by one-third, the slope of the surface of the Connecticut would still have been more

than one foot per mile. This, in a torrent 2500 feet wide, with a depth of 140 feet, would produce a current of eighty miles per hour on the surface and of six miles on the bottom. With this size of the flood, the rate of discharge would be about 400 cubic miles of water per annum; whereas, at the present time, the total discharge of a year is only about five cubic miles. To cause this enormous flood, Professor Dana supposes that, for a short period, the Connecticut glacier melted at the rate of more than a cubic mile per day. As he estimates the area of its drainage-basin to be about 8500 square miles, this would imply that at times as much as eight inches per day melted from the surface, assuredly a portentous hypothesis.

While the fluviable theory seems utterly to fail to account for the various facts of the stratified drift, some inquirers have appealed to the former existence of great fresh-water lakes, whose waters were ponded back by barriers of ice, and which deposited the stratified drift at high levels. They thus account for the terraces of gravel, etc., such as in the notable case of the Parallel Roads of Glen Roy; for the occurrence of kames at great heights, etc., etc. In regard to this view, it seems to have been formulated to meet very local and limited cases, and to ignore the conditions under which the stratified drift occurs in America, in Germany, Russia, etc., etc., where it is impossible to see how the necessary barriers for ponding back the water could be constituted. Sedgwick long ago pointed this out in regard to England. In discussing the origin of the superficial gravels, he urges that they cannot have been distributed, as some had urged, by means of former great lakes, since " in none of the higher parts of England out of the reach of ordinary floods have any traces been yet discovered of lacustrine terraces, such as those which are seen in one or two of the glens of Scotland; or of any other deposits indicating the former presence of extensive tracts of stagnant water." " The hypothesis," he continues, " which ascribes the distribution of the enormous masses of diluvial gravel existing in so many parts of our islands to the agency of a series of lakes, which from time to time have burst their barriers and descended to lower levels, may, therefore, at once be rejected as gratuitous." [8]

[8] Annals of Philosophy, x. 18-19.

Turning to Scotland, where the theory has chiefly flourished, Jamieson says: "Inasmuch as the stratified beds are found terraced not along a few rivers, but along all that I have examined, without exception, and likewise along minor streams, I think the position very improbable, for it can hardly be thought that there has been a bursting of lakes in all the watercourses; and, indeed, with the exception of accumulations of water dammed up by glaciers and the like, such bursting or suddenly giving way of a natural lake is an event of the greatest rarity, nor do I remember having read of an instance of it." [9]

Speaking of the deposits in Glen Derry, he says: "The great height of the mounds, the absence of any barrier below, and the fact that the upper limit of the deposit seems to be higher than the water-shed that divides the head of this glen from that of the Alt-Dhu-Lochan, whose waters descend into the basin of the Spey, seem all adverse to the idea of a fresh-water lake." [1] Again, in regard to the deposits in the Tummel valley, he says: "The part of its upper limit overtopping all barriers between it and the sea is decisive against its fresh-water origin." [2]

In discussing the stratified drift of Aberdeenshire, the same writer tells us how he examined, by means of a spirit level, the height of the terraces on the banks of the Spey. He found the terraces on both sides of the river to be at the same height. "Turning my view," he continues, "up the river to a similar bank opposite Rothes, I ascertained that it was several yards higher, after allowing for the curvation. I then swept the river towards the Moray Frith, and found (what was very apparent even to the naked eye) that I quite overlooked everything between me and its waters. It thus became evident that here was no fresh-water deposit accumulated in the depths of some dammed-up lake. No remnant of any barrier appeared that could account for a lake at such a height; no glacier descending from Ben Aigan or any hill on the east side could have barred the valley down here; for there is no height on the opposite side that the icy avalanche could have rested on . . . the fact remains that these banks rise in height as we ascend the valley. Those at Rothes are

higher than those at Cairnty; and similar terraces which I observed above Aberlour, and also opposite Ballindalloch, seemed to be higher still." [3]

Lastly, the glacier-lake origin of the Parallel Roads of Glen Roy seems to be put out of court by the facts established by the careful levellings made for the six-inch survey map, which prove that the terraces are not horizontal as they ought to be if they had been lake margins.

On this subject a most competent authority, Sir Henry James, urges, that the difference in level between the terraces of Glen Roy and of Glen Gloy of twenty feet, can hardly be accounted for on such a theory, nor are there similar terraces in the neighbouring glens as there ought to be on this theory or on that of marine submergence, to which we shall presently turn.

Nor are the terraces like the margins of ordinary lakes, the absence or scarcity of rolled pebbles in them being a marked feature, the fragments of rock in them being, as Mr. L. Lyell describes them, sub-angular, little weathered and like sub-aerial detritus. Apart from this, I confess that I cannot understand how the necessary ice barrier could be produced to pond back such a lake as is postulated. As Mr. Bonney well puts it, " the district about Glen Roy is not materially lower than the hill region to the west, and the rainfall though less heavy, is considerable. To form large glaciers in the region of Ben Nevis would require a temperature at the coast not higher than 30°. If so, the temperature on the hills above Glen Roy would not rise above 26°. If then the cold sufficed to form glaciers large enough to choke Glen Roy or Glen Spean, there would be glaciers also in Glen Roy itself, and it would be filled with ice, not water." [4]

If the lake theory fails to explain the terraced drifts, much more does it fail to account for the widely-spread mantles of gravel, the beds with marine shells, the kames and the eskers, etc., etc.

Having discussed the various fresh-water theories, let us now turn to those writers who invoke marine forces as distributing the drift.

Mr. Trimmer, in a paper on the "Erratics of Norfolk," as

[3] Q. J. G. S. xiv. 526-7.   [4] Proceedings Geol. Soc. 1891-2, 28.

long ago as 1851 discussed the various facts which are forth-coming in the eastern counties, and concluded that we have evidence of nearly the whole of England having been submerged during the erratic period.[5]

Professor Ramsay, writing in 1852, and describing some of the drift beds of North Wales, concludes : " When we con-sider the continuity of these beds with the shell-bearing strata, their regular smoothly sloping outlines, and their gradual change from gravelly drift with a few scattered boulders on the coast, to the coarser and more massive accumulations among the mountains, and, further, if we add to this the travelled boulders and masses of rock on the summits of hills and ridges 2300 feet high, it seems to me impossible to resist the conclusion that the whole material from the present sea margin upward is of marine origin, and due to the operation of one general set of causes extending over a definite period. . . . . After the great glacial period much of the country was depressed beneath its present level at least 2300 feet, by which the glacier markings were covered by accumulations of super-ficial detritus. The higher parts of Wales, at the utmost from 800 to 1000 feet above the water, must have formed a group of islands, perhaps too insignificant and low to admit of the formation of glaciers on their flanks." [6]

In 1853 Professor Phillips argued that in a comparatively modern geological period, every part of Yorkshire below the level of 1500 feet was covered by the waters of a glacial sea, and that the assigned depression of 1500 feet affected the land far up towards the north and to the east and west, but ceased or grew much less towards the south.[7]

Mr. Jamieson, a great champion of land-ice, writing in 1860, says : " The many instances I have adduced of high-lying stratified drift and transported boulders, at elevations exceed-ing 2000 feet, indicate an almost total submersion of the country ; " and he quotes in proof the many instances of high-lying boulders in the south of Scotland adduced by Maclaren, Chambers, Nicol and others, notably one referred to by Darwin on Ben Erin, at a height of 2200 feet, and adds, " In the High-lands of Perthshire and Aberdeenshire I found the presence of far-travelled boulders at heights exceeding 2000 feet by no

[5] Q. J. G. S. vii. 26.　　[6] Id. viii. 373-4.　　[7] Brit. Assoc. 1853, p. 64.

means uncommon, and that it was rare to find any extensive ridge without them, showing the great submergence was not local but general over Great Britain. . . . I think the tendency of the evidence points to an extent of submergence during the drift period that must have quite extinguished all the larger mammalia ; and as no upper limit has been established, perhaps it may have been so complete as to annihilate all terrestrial life in these islands.

" In the Central Highlands of Braemar, the higher mountains are generally remarkable for tremendous precipices on their sides, most frequently on their eastern flanks. Some of these stupendous cliffs rear a perpendicular front of 1000 or even 1500 feet, as at Loch Nagar and Cairntoul, and it will be found that their base is frequently at an altitude of 2000 to 2500 feet. Wandering along these lofty walls of granite, the thought sometimes struck me that in them I saw the ancient sea cliffs of this glacial sea, and that here its chill waves were stilled." [8]

Sir A. Geikie urges that " the mounds of sand and gravel, the huge erratic blocks and the sheets of clay and silt forming the stratified drift, were accumulated while the land was rising from the sea. In the higher parts of the island the evidence of submergence is chiefly furnished by mounds of water-rolled gravel and sand, and by the dispersion of the erratic blocks. These materials may, in some instances, have been deposited by rivers flooded by rapid thawings of the ice and snow of the uplands. Yet in other cases, although not associated with marine organisms, they are seen to be in places far out of reach of any stream, and on which only the waters of the ocean could have rolled. Beds of clay of marine origin are chiefly found at lower levels, especially close to the shore, whence they descend beneath the sea. It is in these clays, and more rarely in the mounds of sand and gravel which overlie them that the organic remains of the stratified drift occur." [9]

I might have quoted from many other writers, for the view that a large portion of the drift beds have been arranged by the sea during a period of submergence succeeding the general glaciation of the country. These will suffice, however. I will

[8] Q. J. G. S. xvi. 369-370.
[9] Trans. Geol. Soc. Glasgow, vol. i. part II. 145.

now proceed to criticize this conclusion which seems to me to fail to meet the facts.

Trimmer, who was a great champion of submergence, admits as remarkable facts, the general absence of marine remains from the upper erratics of Norfolk, and the general absence of regular beds of these remains from both the upper and lower erratics in every district which he had examined, and he says he had examined many in England, Wales and Ireland.[1]

Jamieson, who will be everywhere accepted as a very learned and high authority upon the Glacial beds, and who, as we have seen, had committed himself to a theory of widespread submergence, in a paper on the last stage of the Glacial period in North Britain, speaking of the theory which postulates no disturbing influences, save subaerial ones, since the supposed great submergence, says : " In such a case I should expect to find level sheets of gravel, sand, and silt, containing some remains of marine fossils in a more or less perfect state ; also, zones of beach pebbles mixed with some littoral shells, and deposits of a similar nature capping eminences that had been in shoal-water ; and in particular I should look for traces of estuary mud along the curves of the wider valleys, where the tide and the river had formerly met. Now, in Scotland, so far as I am aware, we have absolutely no trace of any such estuary beds containing remains of animals peculiar to places of the sort, except at levels below 30 feet, and which belong, as I have elsewhere shown, to a more recent period, when glacial conditions had passed away, the shells indicating a climate rather warmer than at present. *How could the glacial sea have gradually retired, or, rather, how could the land have gradually emerged, without some tidal sediment being left here and there along the valley where a pause in the change of level took place?* It is true some have thought they have discovered traces of ancient sea margins in certain more or less horizontal banks and terraces, which, however, admit of a different explanation ; but no one, so far as I remember, has been able to point out any estuary beds, containing estuary fossils, along the valleys at high levels. Why, also, should we not see some more distinct lines of old sea-cliffs and sea-caves at higher altitudes, and,

[1] Q. J. G. S. vii. 24.

likewise, some heavy masses of blown sand and shells like what we find on the coast at present? The beds of glacial marine clay and sand have been destroyed along the valleys to an extent inexplicable on the supposition that the sea gradually retired and nothing but ordinary subaerial action followed. In certain low districts, where this clay has nearly all disappeared, patches of it are left on eminences and places just where we might suppose it most likely to have escaped the action of glaciers, and at the mouth of some valleys (as, for example, that of the Dee, at Aberdeen) we find masses of it, which seem to be denuded remains of beds that some powerful agent has swept clean out of all the rest of the valley. And some of these beds appear to have been dislodged from their original position and thrust out seaward in a confused mass, as in the banks near the Aberdeen lighthouse and powder-magazine."[2]

Jamieson argues that the rolled gravel is not the result of ordinary sea action along the shallows of former coast lines ; firstly, from its position, as it is not found as belts or terraces along the sides of the hills, but is chiefly developed in *the middle* of depressed tracts, as if it had been projected down these valleys. Now the action of the sea, or of the waters of a lake breaking upon a shore, is to *heap up* sand and pebbles and pile them along its margin. Secondly, from the arrangement of the pebbles, and the false bedding indicating the action of a current in one definite direction. Thirdly, from its being occasionally thrown together into dome-shaped tumuli and abrupt hillocks of considerable height, such as we do not see on any coast line, and also from its uniform increase towards the mouths of valleys. Fourthly, from the absence of marine fossils.[3]

"In the north extremity of Aberdeenshire," says the same writer, "there is a hill called Mormond, about 800 feet high, a few miles south of the town of Fraserburgh. It rises out of the low surrounding region like a great mole-heap. Many years ago I noted, among other proofs of the presence of the sea, beds of well-rolled shingle, forming the crest of certain low hills at elevations of from 200 to 480 feet, in some of which I got remains of Arctic shells. I argued, therefore, that if the

[2] Journ. Geol. Soc. vol. xxx. pp. 318-319.   [3] Id. 350-351.

sea had covered the land to the height of 480 or 500 feet, it should have encircled this hill of Mormond, and formed a belt of shingle round it at a corresponding height; or, if the submergence was sufficient to completely cover the hill, it ought to show a mass of gravel on the top. I accordingly spent two long summer days in the month of June examining the hill, which is a wide-spreading, heath-covered mass, some eight or nine miles in circumference, bare and brown, without a bush or a tree upon it. I was disappointed and puzzled to find no rolled shingle anywhere over the whole surface of the hill, neither on the top nor on the sides of it. But upon the base and skirts I came upon mounds of gravel which did not form a zone or sheet at any regular level, but were disposed in a manner I could not account for by marine action of any sort. Nowhere did they reach the height of 400 or even 300 feet, and all the tops and sides of the hill seemed completely destitute of marine deposits of any kind—no beds of gravel, shingle, sand or silt—nothing but the angular stony rubbish of the gneiss, quartz rock, and granite of which the hill is composed." [4]

Jamieson further argues that if the kames had been formed under the sea they would have contained more sea-shells. "I have also a difficulty in believing that narrow, crooked (sometimes even zigzag) ridges of gravel, with their sides sloping at angles of 20° to 30°, would be formed beneath the sea, or, at all events, could have emerged from it without having been thrown down. I should think the materials could not lie at such an angle of repose beneath agitated water, and would have been levelled to some extent as they came under the influence of the waves and breakers. They could not have stood the dash of the water in stormy weather, even in comparatively sheltered places. The material of which these ridges are usually composed is of a loose incoherent nature, and the present slope of their sides is often as steep as what the sand and pebbles can lie at. Now the angle of repose of such stuff in strongly agitated water is much less than in air. Long, narrow mounds sloping steeply on both sides could not have preserved this form when lashed by the waves and breakers; and I therefore maintain that when the movement of emergence

brought them near the surface of the water they would have been levelled to the angle of repose which gravel beaches usually exhibit." . . . Speaking of the possibility of their having been made by icebergs or pack ice, he says : " Unless the mounds were formed high up on the beach beyond the extreme limit of the tide, they would be levelled again by the action of the waves. If formed in this way they should also contain some littoral shells."

Dr. Scouler, having pointed out how certain ravines near Dublin are completely destitute of any vestige of marine gravel, although it ascends to higher levels in the neighbourhood, said he could not understand on what principle of selection one set of hollows had become receptacles of this shelly drift while others had escaped.[5]

Mr. Oldham, in 1848, was struck with the same remarkable fact, namely, that several of the glens in Wicklow contain no traces of marine beds, although shelly gravels surround them on all sides and ascend to much higher elevations.[6]

The question has been examined with some patience and care by Mr. Dugald Bell, in a paper in which he has quoted some of my own remarks. In this he says there is good proof of some submergence during the glacial epoch, say, of some few hundred feet, but he disputes that there is any sufficient evidence of a submergence to the extent of 2000 feet or anything approaching it. " There are no distinct or traceable old coast lines," he says, " at all approaching to such a height ; whether cliffs with their sea-worn caves, or shelving shores with their zones of beach pebbles, or receding bays of beds of gravel, sand, and silt mixed with marine fossils," and he disputes the conclusions of the late Mr. R. Chambers, who, in his book on " Ancient Sea-margins," had professed to find them as high as 1000 feet above the sea-level. " Some of his supposed sea beaches are lines of weathering where the strata are horizontal or nearly so, others are flutings formed by the action of glaciers, others are river deposits, and others again mere margins of ancient lakes." [7] Judging by beaches or coast lines, Mr. Bell concludes, " There is no indication of anything like the submergence contended for, and the entire

[5] Journ. Geol. Soc. Dublin, i. 256.   [6] Id. iii. 302.
[7] See Geikie, Great Ice Age, 297.

absence of these evidences on the supposition that such submergences really took place, is not satisfactorily accounted for by any theory which has yet been broached for the purpose."[8]

Turning to the beds with marine shells, the first thing that strikes a critic is the extremely local character of the marine drifts. They are found in isolated places, and only found in places bordering on the present sea. This very important factor in the problem did not escape Sir Charles Lyell. Speaking of the shell-beds at Moel Tryfaen and those in Vale Royal, he says: "The two localities are about eighty miles distant from each other in a straight line, and the Vale Royal shelly drift is near the watershed of the centre of England. Intermediate between these points there are areas varying greatly in height above the sea, composed of every description of rock, sometimes covered with drift, but often free from it, and where proofs of marine submergence are entirely wanting. These have been surveyed with such care, that, but for the occasional patches before mentioned, in which the shelly remains occur, a geologist who relied on negative evidence might have confidently affirmed that the land had not been covered during the formation of the drift by salt water."[9] But this is looking at the difficulty through the wrong end of the telescope, which only minimizes it exceedingly. If we are to gauge its real proportions, we must go much further afield. We must inquire how it is that these drifts with shells are not found in the interior of Great Britain, and only in isolated patches along the coast. Surely if the land were generally submerged, as is generally assumed, even for a comparatively short time only, we should find shell-beds in various parts of England, and not merely in the Severn Valley and on points near the sea.

Mr. M. Reade says: "It is a striking fact that though the whole of the Pennine chain has been under water, only at one place, near Macclesfield, 1200 feet above the sea, have marine shells been found, and those mostly fragmentary. The same with Wales, the same with the lake district; these organic evidences of subsidences are only sporadic."[1]

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[8] Trans. Geol. Soc. Glasgow, ix. 101-102.
[9] Antiquity of Man, p. 318.   [1] Trans. Geol. Soc. Glasgow, p. 275.

Let us now turn to the marine drifts in the valley of the Severn, which have given rise to the hypothesis of an arm of the sea having once occupied this area, to which the name of the Straits of Malvern has been given. Of the shells in these beds Mr. Maw says he could not find that any species are peculiar to particular parts of the deposit, but the fragments of shells are distributed throughout the whole mass of drift, including the clay and gravel beds. They are very fragmentary, only six or seven being perfect out of several hundreds. "Of that massive and strong shell, *Cyprina Islandica*," he says, "I have detected nothing but fragments scarcely an inch across and mostly much broken. The broken and water-worn condition of these remains would support the idea of their long transit from perhaps northern latitudes ; but the evidence before us of the repeated tearing up and redeposition of the beds in which they occur would also account for their fragmentary state. . . . At the point where the drift beds rest against the old coast-line of Wenlock Shale I made a careful examination of the water-worn surface, with the object of ascertaining if some of the shells occurred *in situ*, but found nothing different from the usual state : all were broken and fragmentary." [2] All this surely points most strongly to these beds having been completely moved and re-arranged. If it had been the case of a mere submergence of the area, the shells would have been found *in situ*, and widely distributed.

Whichever way we view these Severn Valley marine beds, we seem to be forced to the conclusion that they are not the remains of an old sea-bottom, but the result of some transient movement which transported them from elsewhere.

The same thing is true of Scotland, thus Mr. Jamieson remarks, that although the marine fossiliferous beds may be traced in many places to a height of 200 or 300 feet above the sea, they are nevertheless totally absent to all appearance along many of the valleys in the interior of the country at much lower levels. "Thus no marine fossils have been met with along the valley of the Caledonian Canal between Fort William and Inverness, although the summit level of that valley is only ninety feet above the sea ; neither

[2] Journ. Geol. Soc. vol. xx. p. 139.

have any been found, so far as I can learn, along the whole line
of the Highland Railway from Dunkeld to Inverness. In
the valley of the Dee we have some patches of this marine
clay and sand of great thickness in the neighbourhood of the
town of Aberdeen, close to the mouth of the river, but they
vanish before we get a couple of miles up the valley, nothing
being found beyond that except gravel and boulder earth.
And along all the mountainous seaboard of the West High-
lands marine fossils are unknown, except in spots close to
the shore, and only a few feet above the reach of the tide.
On the other hand, in the comparatively low-lying districts
of Caithness, north-east Aberdeenshire and Fife, these marine
clays form wide sheets and range up to 200 or even 300 feet
above the sea." [3]

Mr. Bell, speaking of Ireland, says : " The greater part of
the country is covered with drift, which is local in its origin," and
he adds, " It is singular that no fossils have been noticed in the
gravels of so great a part of Ireland.   A few *Mytili* in Sligo, a
Buccinum in Moate, Co. Westmeath, and a few fragments in
Tipperary, appear to be all that have been seen."   Yet " at
Boreragh, in Derry, the clay rises to a height 1150 feet above
the sea, and contains many fossils of few species.   These are
Turritella terebra, Cyprina, and *Leda oblonga.*" [4]

Ireland, therefore, shares with Great Britain in having the
greatest part of its surface free from traces of marine organ-
isms.

In America, in opposition to the notion of a general sub-
mergence, Dana says, " south of the latitude of Hudson Bay,
no shell-bearing sea beaches occur above the level of 500
feet.   Nor are there marine deposits or shells over the interior
of the continent.   The greatest height of shore-shell-beds in
or near the United States is 470 feet, and this occurs on the
St. Lawrence.   Nothing of the kind occurs in Ohio nor south
of the river." [5]

But this again is only a small part of our problem.   How
comes it that no marine shells, except near the coast, are
found in the drifts of the continental districts ?   How is it
that they should be universally barren, except on the shores

[3] Q. J. G. S. xxi. 171-172.   [4] Geol. Mag. vol. x. pp. 448-9.
[5] Id. 533-4.

of Scandinavia, and the southern margin of the Baltic, the Atlantic, and the North Sea? Here Mr. Belt has made some very judicious observations : "Excepting around the southern border of the Baltic," he says, "and just so far as, and no farther than, the Scandinavian glaciers reached and carried up fragmentary shells from the arms of the sea they had crossed, the northern drift does not contain sea-shells or any other marine organism. For thousands of square miles, south of the irregular line I have indicated, up to and around the Carpathians, the northern drift is spread out, and not a trace of marine life, not even a diatom, has been recorded from it, while at its base, between the Oder and the Elbe, freshwater shells abound. To believe that Europe gradually sank down below the level of the sea until the latter had its shore-line more than 1000 feet up the flanks of the mountains, and that it rose again without the sea leaving behind it any traces of life excepting freshwater shells, is such an extreme hypothesis, and so contrary to all we know respecting the composition of existing sea-bottoms, that it is probable that its present acceptance is simply a survival from the time when there was no other way of explaining the existence of water up to such a height. . . . There is much evidence to show that vast continental areas were never below the sea level from the close of the Palæozoic period up to the end of the Tertiary period. Yet after this stability of surface over such an immense period of time no hesitation is felt, in the comparatively insignificant Glacial period, in sending the surface of the land thousands of feet higher that ice might accumulate on the now low ranges, and, thousands of feet lower, that icebergs might float over the submerged lands ; and no difficulty is experienced in believing that it should finish its wonderful oscillations by regaining the level it had before the Glacial period commenced. It seems a burlesque on science that such theories should be prevalent amongst our geologists, and if they were not held by philosophers, they would be ridiculed as unphilosophical. Those who advocate the former existence of these oscillations of the surface are those who urge that we should not call in the aid of any but existing agencies ; yet where do they now find a shore-less and a shell-less sea? Put down a dredge

anywhere in the ocean within depths of less than 2000 feet, and in the small quantity of clay, mud, sand, or gravel scraped up, it will be scarcely possible to take out a teacupful that shall not teem with marine organisms; yet we are taught that an immense area in Europe and America has been a sea-bottom, and every part of it a sea-beach as the land rose again without any existence of marine life having been left behind." [6] This admirable passage, as it seems to me, is conclusive, but I will supplement it by another by the same talented author, the value of whose ingenious work has not been sufficiently recognized. In this case he refers to Siberia, which is so continuous in regard to its physical aspects with the great plains of Russia, that it is hardly credible the latter should have been submerged for any length of time beneath the sea without it also having been subjected to the same influence. In answer to Professor Bernhard von Cotta, who postulated a submarine origin for the surface of Siberia, Mr. Bell says, "I believe that the absence of sea-shells is fatal to the marine theory : I searched diligently for them and could find none; and excepting in the extreme north, around the present coast, I believe none have been found by other observers. The mollusca exist all over the present ocean; they abound around Greenland even within a short distance of the foot of the great glacier. I do not contend that the presence of shells of *Cyrena fluminalis* is a proof that the waters in which the sands were deposited were not marine, as they might have been (and I believe were) brought down by streams from the south; but their preservation in the sand proves that marine shells did not exist there, or their remains would also be found. I have worked most of the coasts of the world, and dredged in northern waters, and everywhere found marine molluscs to abound, and I believe that the absence of sea-shells in any Tertiary or Post-Tertiary strata, excepting in some muddy deposits in which they could not well live, is a proof that these strata were not deposited in the great ocean." [7]

These difficulties are assuredly most embarrassing for those who conclude that the shell-beds point to the positions where

[6] Quart. Journ. of Science, vol. vii. pp. 82-83.
[7] Journ. Geol. Soc. vol. xxx. p. 495.

they are found having once been the sea-bottom, and especially for those who argue that its submergence and re-emergence were the result of causes operating for thousands of years. We must remember that if Sir Charles Lyell's calculations are of any value, they understate the problem, for many of the shells found in these beds are not littoral, but deep-water shells, and the land must not only have sunk to the level of the sea, but much below, to render it a fit habitat for them. We are to believe, then, that a whole continent, which was submerged for ages in this way, has nothing left to show the fact save these shreds left at different levels and always near the present coast-lines. In regard to the differing levels at which they occur, my acute friend, Mr. Darbishire, speaking of the Moel Tryfaen deposit, says :—

" It is difficult to conceive of the deposit of a continuous bed of shingle 600 feet deep, with precisely similar fossils in its highest and lowest layers, and of the removal of the whole of the formation, except a few patches of each layer lying within a space of six miles. It is more difficult to suppose that the Cemetery beds (i.e. the beds at a lower level) can be a redistribution of such portions of Mr. Prestwich's gravel as the wave of a retreating sea carried away while the land was rising. It is scarcely more easy to believe that the Cemetery beds and those of the higher land are merely portions of a deposit under similar conditions on a rising coast, the incline being not less than 600 feet in six miles." [8]

The mixture of the shells is another remarkable feature in such of the beds as contain shells. There is no more interesting and romantic place in the world for the student of recent geology than the famous inlet at Uddevalla in South Sweden, whose shell-beds have been so much used in their works by Lyell and others. I have visited these beds twice, and examined them with considerable care, on the last occasion with my friends Mr. Robert Darbishire, F.G.S., and Professor Marshall, and in the company of Mr. Dickson, who has done so much to work out their contents. It seems to me that the story furnished by them has hardly been sufficiently realized. I will quote a description of them in the admirably graphic words of Linnæus, who first

[8] Mems. Manc. Lit. and Phil. Soc., 3rd series, vol. iii. pp. 65-66.

called attention to them, which have been translated into equally graphic English by Dr. Latham :—

" The shell hills (Skalbargen) are rightly reckoned amongst the greatest wonders of Bohuslaen ; for they lie inland near a whole quarter of a mile, in some places, from the sea. These shell hills consist of periwinkles and bivalve shells (Snacke-och Muskle-skal), which are here assembled in such numbers that one wonders how so many living beings existed on the earth. We visited Capell Hill, which lay a quarter of a mile beyond the southern Uddevalla Gate ; then we went to Sammered, which lay nearly a quarter of a mile from the town, north-east. In both places were these shell hills, especially and most markedly at Sammered. Here there were bare and hillocky ridges of grey stone, on the sides which face the town or the sea, where the bay was originally bent in. The earth was slightly convex on the summits of the above-named hill, and made a curve, where the black mould, which was seldom more than a foot and a half deep, thinned off ; the shell-bed, which was two or three fathoms deep, underlaid it. Under this came in succession pure clay. No shells were seen above this stratum. Among the bare hill ridges they stretched, however, altogether from the hill downwards under the black mould, often to the breadth of several gunshots. The shells lay clean and unchanged, with no addition of soil, only strewn over with a little gravel, such as is thrown up on the beaches." [9]

This is a very faithful account of what is to be found here, and assuredly it is a very strange one. To find shells of the most fragile character, perfectly preserved, heaped up in this fashion many feet thick, with hardly any mixture of sand or shingle, quite heterogeneously, the species being mixed together in most admirable disorder, those from deep water being mixed with those which are purely littoral, shells which occur loosely in the sand being mixed with abundant specimens of more than one species of barnacles which are attached to rocks, etc., and of mussels, etc., occurring gregariously in beds. This is assuredly a very puzzling assemblage. In the first place, it is absolutely clear that these shells could

[9] Linnæus, West Gotha Resa, pp. 197-8; Memoirs Geol. Survey, vol. i. p. 364.

not and did not live where they are found. They could not have lived in heaps such as these. Shells having such very diverse habitats could not have lived together mixed up in this heterogeneous fashion, bivalves and univalves all huddled confusedly together in myriads. This is absolutely plain.

This has been noticed of the shell-beds elsewhere. Speaking of the Moel Tryfaen shells, Mr. Forbes says, " I have lately examined them carefully with a view to see whether they indicate an ancient coast-line and beach, or an ancient sea-bottom. But they cannot be regarded as indicating either, being a confused mixture of fragments of species from all depths, both littoral and such as invariably live at a depth of many fathoms—of such species as *Astarte elliptica, Mytilus edulis, Tellina solidula, Cardium edule, Venus gallina, Buccinum undatum, Mactra solida, Dentalium entalis, Cyprina islandica,* and *Turritella terebra*—inhabitants, some of muddy grounds, some of sandy, some of rocky. Deep and shallow-water species, mingled, could at no time have lived together or have been thrown up on one shore." [1]

Mr. Mellard Reade makes similar remarks in regard to the Lancashire drift shells. Thus he says, " The association of the various species, distributed entirely without order through the clays, shows that they could not possibly have lived together on the same bottom, some being peculiar to sand, others to mud, some to rock, others to shingle, some requiring deep water and others shallow; so that the conclusion is irresistibly forced upon us that they must have been to a large extent transported." [2]

Again it is remarkable that there should be no evidence of this alleged submergence in the south of England. According to Lyell, " The district south of the Thames and the Bristol Channel seems to have remained land during the whole of the glacial period at a time when the northern area was under water." [3] As Mr. Bell says, " It would be a very surprising result if while all the land north of the Thames and Bristol Channel and that of Ireland was submerged till only the summits of the highest hills appeared as scattered islets

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[1] Op. cit. p. 384.   [2] Journ. Geol. Soc., vol. xxx. p. 32.
          [3] Antiquity of Man, 275.

above the sea, all south of the Thames and Bristol Channel remained clear and dry above water. In North Wales there was a submergence of 1400 feet, while at a short distance to the south of the Bristol Channel this great depression died out or disappeared, so that there was no submergence at all. Is this credible? . . . can we suppose such an abrupt termination to an actual submergence of the land? that it could be so great immediately to the north of the Bristol Channel, and nothing at all immediately to the south of it, and this without any apparent break or dislocation in the strata along that line?" [4]

In regard to the possibility of a local depression limited to Wales and its borders, Mr. Bell quotes Lyell to the effect that the horizontal carboniferous beds of the borders of Wales and of Shropshire have never been violently fractured or bent into folds, whether by sudden or continuous lateral pressure, since they were deposited so that in the strata there is no evidence of a local disturbance since carboniferous times." [5]   " It would," he adds, "be surprising enough that the whole country should be depressed and re-elevated 1500 or 2000 feet without any apparent disturbance of the strata, but it is impossible to conceive only some parts of it affected in that manner, and to that degree without any such results being produced." [6]

It is not only in the south of England where we have slight if any traces of the submergence. " No raised beaches or pleistocene marine formations are found in the Shetlands and Orkney islands above the present sea level." [7]

It is difficult, again, as Mr. Bell says, to believe that North Wales and part of Ireland were more deeply submerged than other parts of the northern hemisphere. " If these beds were really laid down by the sea, we ought to find similar deposits at corresponding elevations in many other localities. If the sea once stood at that height there must have been innumerable sheltered bays and inlets, narrow fiords and broad channels at a similar level all over the country, in which marine organic remains should be found as well as here. How is it they are restricted to such a few spots, and that

[4] See Bell, Trans. Geol. Soc. Glasgow, i. 127-128.
[5] Id. 125.   [6] Id. 129.   [7] Id. 137.

none occur at levels between 1300 or 1400 feet and those at 510 feet?"[8] The usual reason given is that they were removed by the second or later glaciation, which is chiefly summoned to remove the traces of the supposed submergence. "It is unlikely that any well marked coast lines at various heights should have been all smoothed down and effaced by this 'later glaciation' . . . for the sea has a very devious, sinuous coast line—at any level it must always have had such a coast line—winding into every recess and angle, into every little side valley and sheltered nook of the country; whereas a glacier keeps as straight as possible to one main channel, and disregards side openings, which the sea does not. . . . The glaciers could not therefore make a clean sweep in the way suggested, for they could not branch off or flow out into all the curves and bays and recesses that were occupied by the sea. Whatever their action might be as far as it extended, it is simply impossible their area could coincide with that of the submergence.[9] Again, the sea makes a line of uniform level, a glacier a descending line, so that in coming down a valley a glacier might sweep away the sea beach at the upper part of the valley, but not at the lower end of it. The margin lines of a glacier would soon cross and run under those of the sea which would then remain intact."[1] Nor can we believe that the traces of these old beaches have been removed by ordinary sub-aerial denudation, for nothing would resist its effects more completely.

In regard to this notion of denudation, Mr. Geikie says: "If arctic shelly clays ever occurred in as thick beds in the inland as in the maritime districts, surely we should have found some notable trace of them. It will not do to lay the blame of their disappearance on that geological scapegoat *denudation.* Denudation has not run off with the kames. Why should it have been less considerate with the clays? The kames have come down to us almost, if not quite, in the same state as the sea-god left them; but if shelly clays ever existed in the interior parts of the country, they would appear to have vanished, and left not a wreck behind. If it were in Scotland only where the marine shell clays were confined to the maritime districts, there might be some excuse for

[8] Id. 108.   [9] Id. 108-110.   [1] Id.

dragging in denudation to account for their absence at the higher levels reached by the kame drift, but in Norway and Labrador and Maine the shelly clays are restricted precisely as in Scotland to the vicinity of the seacoast." [2]

Another argument has been used effectively by Mr. Belt in the following sentence, in which, although he has somewhat minimized the proportion of unbroken shells in the drifts of which a few certainly occur, yet the main contention seems to me most just. " Where," he says, " was the shore of that mythical sea under which England nearly to the Thames is supposed to have been submerged? How is it that not a single undisturbed bed of Glacial shells has been found, that, nearly all are broken to pieces, that many fragments of *Cyprina* exhibit Glacial scratchings, and that not a single instance has been recorded of the two valves of a Lamellibranch having been found together? Was there no friendly cliff or cavern able to preserve a single shell from the ruthless second advance of the ice? Mr. James Geikie finds the fragile bones of water rats and frogs in his 'interglacial' beds, and uninjured land and fresh-water shells occur in abundance, but not one marine shell has been found."

In regard to the pebbles and boulders found with the shells at Moel Tryfaen, some of them were probably derived from the far north, among them several kinds of granite from Cumberland and Scotland, including pebbles and good-sized stones of decidedly Eskdale granite; the latter must have travelled 130 miles and occurs in the Lancashire drifts.

Mr. Mackintosh,[3] who believed in the great submergence assisted by floating ice, says, " These boulders must have been carried to the top of Moel Tryfaen by rafts of coast-ice." He frankly confesses, however, that there is some difficulty in seeing how coast-ice could pick up granite from the Eskdale fells at a height of nearly 1400 feet above the present sea level. " We have a further difficulty," adds Mr. Belt, " in seeing how, having picked it up in Eskdale, coast-ice could carry the granite to the shoulder of Moel Tyfaen. Would there not be coast-ice there as well as here? How then, and by what skilful pilotage could coast-ice from Eskdale reach Moel Tryfaen?" [4]

[2] Geol. Mag. vol. ix. p. 28.   [3] Geol. Mag. 1872.   [4] Id. 116-117.

The present distribution of the terraces points the same moral. If the land had been subject to secular movements of upheaval and subsidence on a large scale, they ought to be on a tolerably uniform level. As Mr. Jamieson said in a recent number of the Geological Magazine in reference to a very different theory, " The facts indicate a very unequal amount of submergence in places situated in the same latitude. The best-known high-lying marine beds of New England and Canada lie between lat. 44° and 52°, which corresponds to that of France and the South of England, just where evidence of submergence is conspicuous by its absence. Again, we find in Scandinavia evidence of submergence to the extent of 600 or 700 feet, but in the same latitude along the eastern side of the Baltic and in Finland no evidence of like submergence has been found. In the neighbourhood of Dublin, Lancashire and North Wales, sea shells are found in the superficial beds of sand and gravel to heights of 1200 and 1350 feet, but no evidence of submergence to anything like this extent has been detected on the eastern side of England or the neighbouring parts of Europe. In Canada the marine shell beds reach up to 470 feet at Montreal, but, says Mr. Thomas Belt, ' going eastward from Montreal the elevation of the marine beds, marking the former submergence of the land, gradually decreases until in Nova Scotia it reaches zero.' Dana, in the 2nd edition of his ' Manual of Geology,' tells us that the altitude of the marine deposits on the southern shores of New England is 40 or 50 feet, at Lake Champlain (which is in the same lat. as Nova Scotia) they occur up to 393 feet. . . . . In Scandinavia the highest lying shell-beds are in the southern half of the peninsula, where they attain an altitude of 500 or 600 feet, but on going northward from Trondhjeim they seem to decrease in elevation, and in Finmark none have been discovered at nearly so great a height. At Hammerfest, according to M. Bravais, the highest of the old sea-beaches is only 92 feet. . . . Moreover, Bravais found that the old sea-beaches in Finmark are not horizontal, . . . his uppermost beach-line declines in level from 221 feet at its southern extremity in Altenford to 92 feet at its northern end at Hammerfest, thus lowering in level from south to north." [5]

[5] Geol. Mag. Sept. 1882, pp. 401-402.

Lastly, it is very difficult to account for the far travelled erratics by any theory of submergence.

It is a well-ascertained fact that deep-sea currents exert no transporting agency whatever, and that the smallest fragments only of sand, mud, and shells, remain at the bottom of such deep seas and lie in an unruffled state. It is the action of wave breakers alone that abrades coast cliffs, and if such lands had not presented cliffs to the waves, no serious wearing away of them would occur.[6]

I have now examined, I hope with fairness and candour, the evidence of the high-level marine drifts containing shells found at so many scattered points near the coasts of Western Europe and Eastern America. I cannot find them testifying anywhere to those gigantic vertical movements of the earth's crust over immense areas and within quite recent geological times, which are required by the theory of a long-continued submergence advocated by Sir Charles Lyell and his followers, and against which Darwin protested.

This completes the survey of the various diurnal methods in which water works. They all fail to explain the facts. It is reasonable therefore to conclude that if, as is almost universally admitted, we must have recourse to water, in order to explain a large portion of the drift phenomena, we must have recourse to water acting in some other than its diurna methods.

[6] Murchison, J. R. G. S., 39, cxci.

CHAPTER XVIII.

THE DISTRIBUTION OF THE DRIFT CAN ONLY BE EXPLAINED BY INVOKING A GREAT DILUVIAL CATASTROPHE.

After enlarging on the rapid accumulation of facts in geology, Dr. Whewell continues: " To our knowledge of causes we can look with no such certainty of its progress being steady and rapid, or rather, we are certain that the advance must be slow, and may be often and long interrupted. For it is not an advance to suggest one or another hypothetical cause of change, without assigning the laws and amount of the change, it is hardly an advance even to calculate the result of an hypothesis on assumed conditions. To obtain by induction from adequate facts the laws of change of the organic and inorganic creation, this alone can lead us to those discoveries which must form the epochs of geological dynamics."—Proceedings, R. G. S. iii. 96-97.

In invoking a flood to explain the drift, we are appealing to a cause whose reasonableness and necessity have already been established by biological and archæological evidence and arguments—The drift-beds the results of one great movement, and not separable into various horizons marking different periods—*A priori* arguments—W. Hopkins, Dana—Arguments drawn from the beds themselves—Europe—H. B. Woodward, M. Reade, Bonney, Maw, Searles Wood Jun., Goodchild, Mackintosh, Bennett, Bulman— America—Agassiz, Dana—Continuous blankets of drift over wide areas with a soft rolling surface mark the operations of water moving on a wide scale—The irregular margin of the drift points to the same cause—The spread of the drift, irrespective of the surface-contour and in terraces, points the same lesson—So does the distribution of the drift containing marine *débris*—So does the mixture of *débris* from different districts in the drift—The predominance of local *débris* in each locality does the same—So do the great curves and other features of the contorted drift—So does the false bedding of many of the beds—Views of De Luc and Dana on this point, and on the alternation of rough gravel and finer beds, the existence of gravel terraces, etc.—The kames, eskers, etc., also evidence a great flood— So does the phenomenon of " Crag and tail "—So does the great depth of the drift in many places, and especially at the heads of many valleys—The occurrence of boulders without sand, and their gradual displacement by sandy deposits as we go south point the same lesson, so does the distribution of the boulders—Waves of translation—Scott Russell, Hopkins, Whewell, and Rankine—Instances of great stones carried by floods—Bewicke, Basil Hall, Culley, Prestwich, Jamieson, F. Smith, Scrope, Koch, Tylor, G. Austen, Blandford—Abich—A great flood would account for the transport of the chalk masses at Cromer, etc., for the arrangement of the blocks in diverging lines, and for their diminishing in size as we recede from their point of origin—Murchison. For their being

massed in some places and absent in others—For their resting sometimes upright—For their being transported up hill and over heights—Cumming; and for their occurrence as perched blocks— Large rounded blocks must have been transported by floods in former geological periods—Hopkins—Similar blocks in the tropics were similarly transported—Schomburgh, De la Beche, Darwin, etc. On the striæ crossing plateaux, etc.—Detritus dragged along can produce striæ—J. Hall—Koch—Murchison—Mallet and Oldham —Probability that this was the cause of the plateaux-striæ— Hopkins — Concession of great floods by various geologists — Andrews — Hildyard and Dana specially quoted—Murchison— General conclusion.

WE have reached this point in our argument. We have shown that in the so-called glacial period very considerable glaciers occupied nearly all the mountain ranges of Europe and North America, and that these glaciers did precisely the same kind of work, except on a wider scale, that is being done by existing glaciers, that they polished, rounded, and striated rock surfaces and distributed boulders, etc. Outside these glaciers, we have seen reason to believe that land-ice could not, and did not, work as the advocates of ice-sheets argue, and that the various effects which the extreme school of modern glacialists attribute to ice are inconsistent with its capacity and power to produce.

We have further found in common with other inquiries that many of the drift phenomena can only be explained by an appeal to water, and we have discussed the various methods in which water usually works in nature, and have seen that none of them will account for the facts.

If, therefore, we are to appeal to water, and no other cause is available, we have no alternative but to appeal to it when acting in some exceptional and abnormal way. This appeal seems to be an absolute necessity of the case, and having no metaphysical theory to support, and being devoted to no other kind of science than that which is based on induction, I have no hesitation in making it, and am further convinced not only that this hypothesis will alone explain the more simple phenomena of the drift, but the larger and more difficult problems whose solution, as we have seen, still awaits the efforts of geology.

My confidence is greatly strengthened by the fact that in my previous volume on "The Mammoth and the Flood," I have argued at very considerable length, and think I have

established the conclusion, that the sweeping away of the mammoth and its contemporary fauna can only be explained by invoking a great deluge of water, a flood which is demanded, therefore, by a manifold series of facts outside of the drift phenomena, and which has the supreme virtue of explaining not a selection of, but all our difficulties. In having recourse, therefore, to a flood of waters acting on a wide scale to explain a large part of the facts presented to us by the drift, I am appealing to a cause whose reasonableness and whose necessity have already been established on other grounds.

Not only so, but I am reverting to an explanation which in the earlier days of geology was accepted as not only competent to explain the facts, but as alone capable of explaining them by some of the keenest-witted men who have illustrated our science, distinguished not only for their patient collection of facts, but also for their careful induction. Men whose works are too much neglected in these days of hypotheses, the venturesome audacity of which take us by leaps and bounds from the sober ways of science into cloudland ; men, some of whom, I hope, I may have done tardy justice to in the earlier pages of this volume, and whose disciple and whose champion I claim to be now.

Before facing the immediate subject of this chapter I must first diverge somewhat to consider a side issue. For a long time it has been considered by a school of popular writers that the various so-called glacial beds, the clays, gravels, and sands ranged over one another, or side by side, represent a vast period in time, and a complicated series of events, including among other revolutions several upheavals and depressions of the earth's surface. A more modest school of glacialists, headed by Mr. Hull and Mr. Searles Wood, Junr., has been content to separate the glacial beds into three horizons only, marking three epochs, and respectively referred to as the lower clays, the middle sands and the upper clays.

I am a complete disbeliever in any such separation except as a mere local phenomenon. Whichever way we view the facts, they seem to point to these beds representing different phases of one movement representing one period of no long duration, and the differences in the beds seem to mark, not the operation of different and widely separated forces, but

the manifold handiwork of water which at one and the same time can and does lay down shingle beds in one place, sand-banks in another, and mud in a third, according to the force and character of its currents. This view is not my own only, it is shared to a large extent by some of the most distinguished explorers of the glacial beds. They have pointed out that the same shells occur at all supposed horizons in them, that pockets of the same kind of sands and gravels are found imbedded in the midst of the clays like plums in a pudding, and traversing and overlapping lines of junction and of bedding, and that the beds themselves are ranged in no certain order or sequence, but most irregularly, so that no kind of suggested arrangement suits any two localities.

As the matter is one of considerable importance, I shall freely quote some of their testimonies. First I would refer to an authority or two of weight on the *à priori* side of the issue.

In regard to the variation in the structure of the beds denoting a succession of periods, etc., Hopkins says : " Currents would deposit least on shallow bottoms where *cæteris paribus* their velocity would be greatest, and most in the deeper waters ; and moreover it would be in these deeper waters that the finer matter would be deposited. Thus the existence of beds of finer and in many cases stratified deposits, having more tumultuous deposits both above and below them, as in some parts of North America, does not necessarily indicate a cessation in the more energetic action of the forces of dispersion, but may merely indicate deposition in a deeper sea. If also large angular blocks from distant sites should be embedded in this mass of finer matter, we see an additional indication of a deep sea in which icebergs might drop them. . . . Currents attending waves produced by sudden elevations, greater or less, are necessarily *transitory,* and each can only carry the materials it may transport to certain distances, depending *cæteris paribus* on the magnitudes of the component individual masses, the large blocks being carried but to small distances and the smaller particles to much greater distances. Thus the first wave would produce a layer consisting of the larger blocks near their source, and of fine detritus at the remoter distances. The second wave would produce a

similar effect, and would also carry the blocks of the first wave to a somewhat greater distance, and so on for successive waves. The effect, then, of a succession of similar waves would be the formation, over the more remote parts of the area of deposition, of a bed of finer matter, in the *upper portion* of which would exist blocks, rounded and water-worn by their transit. Thus we should have the phenomena of fine detrital matter below and blocks above, apparently referable to separate successive periods of time, during the first of which one kind of agency should have transported the finer sediment, and during the second another and much more powerful agency should have transported the blocks and coarser detritus, while, in fact, the whole phenomena would be really referable to a repetition of precisely the same agency during the whole period of transport. That period, therefore, except in a limited sense, and not with reference to the whole area of transport, would not in the case now supposed be divided into two, but must be regarded as one single period." [1]

Dana argues very much in the same way. He speaks of the " deposit of materials in water as having varied in the degree of stratification according to the depth of the waters and their rate of flow, and also according to the kind of material thrown in and the rate of its supply. If the earth fell in too great quantities for the waters to work over and arrange, there would have been produced only imperfect stratification, or none at all beyond a levelling of the top; while with a more gradual supply, or a fuller flow of water, the stratification would have been regular and general. Very rapid currents would have made layers in the formation that were obliquely laminated, the hurrying waters pushing up the sands before them as deposition went on; and when the supply of earthy material was very large and the waters abundant . . . the obliquely laminated layers thus made ought to have been a foot or more in thickness, for each such layer is necessarily the result of a single onward rush of the waters. When plunging waves accompanied the rapid flow, the resulting layers would have been composed of wave-like parts, each independently laminated. In quiet waters the

[1] Q. J. G. S. viii., ann. add. xxxvii. and xxxviii.

deposits should have been of all degrees of fineness and regularity down to those of clay." [2]

The *à priori* views here stated have been brought home to many explorers of the drift beds in the field, and especially to the officers of the Geological Survey who have had to examine them over a large area, and there is a general consensus among the most experienced of them now, that it is impossible to arrange them in any order which shall illustrate more than very local phenomena, and that it is hopeless to deduce from them a fixture of successive stages and periods extending over a long geological era.

In a capital paper by my friend Mr. Horace B. Woodward, on the " Glacial Drifts of Norfolk," published in the 9th volume of the " Proceedings of the Geologists' Association," he says, " The subdivision of the beds is a fertile source of discussion, and there is no agreement whatever about their method of formation. The drifts do not behave like other and older formations, for, although they may occur regularly for a few miles, boulder clay over sand or gravel on the borders of a valley, yet all at once the clay plunges to the lowest level of the ground in defiance of geological rules. You may find a clay pit in one field, and a deep gravel pit in the next, and this without the changes we are taught to expect in the form of the ground, and sometimes, where the pits are but a few yards apart, it may be difficult to determine the newer deposit." After stating that the contained fossils in these beds are either derived or very fragmentary, while the lithological characters are very variable, he goes on to say that " even stratigraphical succession affords no sure means of identification for lenticular beds, as sand or gravel occur in both upper and lower boulder clay, and the evidence of erratic blocks, although useful, is no infallible clue, as one may often search the Cromer clay in vain for other stones than chalk and flint." [3] Elsewhere he writes, " It was my duty to map a considerable portion of West Norfolk around Fakenham and Wells, and I had to join up from this area with another tract I had surveyed to the east round Aylsham and North Walsham. At Fakenham I commenced mapping

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[2] Dana on the Glacial and Champlain eras, *passim.*
[3] Op. cit. i.

boulder clay, which presented all the characteristics of the chalky boulder clay, and as I travelled eastwards it appeared to merge into the formation I had grouped with the contorted drift. I could not help it, although I felt sorely grieved and perplexed. The mere separation of clay and mud from sand and gravel was not in itself a very troublesome process, but to try and discriminate upper and middle and lower glacial deposits taxed my equanimity to the utmost. . . . If you glance at the Cromer map, you can judge how little the surface geology, as there depicted, tells of the great contortions which are so conspicuous in the cliffs. My case was similar. Nearly every good section showed that the beds were very much disturbed. . . . For instance, on comparatively low ground, where the general surface evidence indicated clay, and the bordering hills were clearly sand and gravel, with a marked feature, the cuttings would show not merely loam and marl and boulder clay jumbled up together, but large included beds of sand and gravel. Hence a sand or gravel pit might be opened in the midst of a tract marked as clay on the map, where of the existence of such material there would be no surface evidence. . . . However, I frequently sought comfort in the Cromer cliffs, feeling that the contemplation of such a contorted, tumbled, higgledy-piggledy arrangement of beds was enough to confound any field geologist who would attempt to mark out the strata in detail. . . .

" My chief difficulty in reality arose from trying to separate the upper chalky boulder clay from the lower boulder clay, which, in West Norfolk, becomes as a rule more chalky than the bed above, and in the end I had to give up attempting to draw a line. . . . In the country around Fakenham where the later boulder clay passed directly over the clayey lower glacial beds, it no doubt becomes incorporated with them, causing one contorted mass, and we cannot separate the two deposits. . . . The conclusion that might be drawn from these remarks is, that after all the divisions of the drift possess little more than local value. . . . One is sometimes tempted to exclaim that anyone who wishes to have a clear and definite notion of the succession of Norfolk drifts should confine his attention to certain sections. If he wished to

feel confident of the triple division, one series of sections should be visited; if a two-fold division would better agree with his notions, other sections might be selected, and so on. My own opinion as to the drifts has fluctuated, according to different districts which I have been called on to examine, and yet probably no district furnishes better sections of the glacial beds than are to be found in Norfolk." [4]

Again he says, "The individual members of the glacial series contain scarcely any distinctive characters based on organic remains, and the constantly recurring local variations of their mineral character seems to render it almost hopeless to attempt any general classification on mere lithological evidence. . . . From what I have seen of the cliffs south of Cromer, I must demur to the opinion that there is anything like a uniform succession of the beds for any distance along the coast. As a general rule, tough boulder clays occur towards the base of the cliffs, and sand, gravel, and silty beds, more or less contorted in its upper parts; but beyond this there is nothing like uniformity, and various sections could be described where it would be impossible to trace any kind of resemblance. In one at Coston Cliff between Yarmouth and Lowestoft. Sand, gravel, boulder clay and silt beds interlace in endless variety, and I must take exception to an occasional resemblance in the order of superposition of the clays and sands along the east coast, being adduced in support of their being the equivalents of the boulder clay and its underlying gravel bed covering the higher ground of Suffolk. . . . I fear the test of facts will scarcely support so simple a classification as the subdivision of the whole glacial series into upper and lower boulder clays, separated by a mass of sands and gravels. . . . There are two obvious difficulties in the way of working out a uniform sequence on lithological evidence : 1st, The constantly varying character of each individual bed, clays and sands passing into each other, through gradations of clayey sand and sandy clay which, unless you have an absolute continuity of section, renders certain identification impossible; 2ndly, After the coast line is left, the various levels at which the same bed may occur following an irregular basement line as pointed out by Mr. Hull, in his paper on the 'Manchester.

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[4] The Glacial Drifts of Norfolk, Proceedings Geologists' Assoc. ix. N. 3.

Drifts,' complicates the difficulty, rendering it impossible to pronounce with certainty to which division an isolated mass may belong."

Mr. Mellard Reade says, "From the time of Joshua Trimmer until now, attempts have been made to distinguish the various beds of clay, sand, and gravel from each other. Trimmer divides them into 'lower erratics,' 'boulder clay,' and 'upper erratics,' (sand and gravel). Professor Hull more lately into lower boulder clay, middle sands, and gravels, and upper boulder clay. My investigations lead me to doubt the value of any of these classifications, and, what makes it more confusing, there is no real agreement among the supporters of this classification as to which division special beds belong to. Thus Mr. de Rance considers all the shells in my list to be derived from the lower boulder clay, while Mr. Mackintosh and Mr. Shone commit them to the upper boulder clay. As regards the fauna of the sand-beds, I have shown that really no geological distinction can be based on these grounds, while on the other hand I have failed to trace out any stratigraphical connection that would support any of these theories of geological subdivision." [5]

Mr. M. Reade again points out that in a description of the Blackpool section some years previously by Mr. Binney, *no separation* of the clay by sand and gravel is given in one particular part of the section *where there is such a separation now*, and he explains it on the supposition that as the section is now several yards further inland, the separating beds now visible have thinned out seawards, and he suggests the probability that if the beds could be followed inland, the upper and lower clays would be found to coalesce in a similar way. This is made more probable by the sections in the neighbourhood. Thus Mr. Reade describes one where "the boulder clay was divided by very persistent beds of sand, though in places these thinned out and the upper and lower beds coalesced and became one without observable division," in other cases the shingle was divided by patches of boulder clay, while "irregular sporadic patches of sand occurred in the clay." [6]

Similar views were urged by older men, thus :—

[5] Trans. Geol. Soc. of Glasgow, vi. 265.   [6] Q. J. G. S. 1874, p. 124.

In comparing the till or boulder clay at Blackpool and Birkenhead, where it forms one thick bed intercalated with beds of silt, sand, and gravel, with the complicated beds which occur as we near the Pennine chain, Mr. Binney says, " I give this section as an instance to show the difficulty of classifying the drift deposits either by my old arrangement or Mr. Hull's new one. . . . The eastern part of England I imagine is in about the same condition; for after some years' examination of the drift deposits in Notts, Lincoln, York, Durham, and Northumberland, I have not been able to make the sequence of the beds, as Mr. G. V. Wood, Junr., appears to have found more to the south." [7]

After quoting a number of sections, Mr. Maw says :—

" This list of examples might be extended very greatly, but they will suffice to show the extreme difficulty of distinguishing on any certain basis, and identifying in distant localities on mere lithological character, the individual subdivisions of the drift series. If compared with the sections enumerated by Mr. Hull in his Memoir on the " Drift Deposits of the neighbourhood of Manchester," it will be seen that the correlation of the individual subdivisions is utterly hopeless. If, for instance, the Strethill Severn Valley section, where there is the unusual consecutive thickness of 210 feet, is compared with the full series given by Mr. Hull in the neighbourhood of Manchester, it will be found that there is no possible correspondence in the lithological subdivisions. At Strethill there is but one boulder clay separating two masses of gravel, and in the Manchester district one sand and gravel bed intervening between an upper and lower boulder clay. Both series seem to have about the same range with reference to sea level, but to bring them into possible correspondence as to lithological subdivision, it must be assumed, not only that one of the Manchester boulder clays is absent in the Severn valley, but that one of the Severn valley's gravel beds is missing at Manchester." [8]

Mr. Searles Wood, Junr., in the *Geol. Mag.* vii. p. 17, controverts the notion of two clays being separated generally by sand, *inter alia*, he says, " Mr. Binney adheres to the opinion originally advanced by him that, although sand ex-

[7] Geol. Mag. iv. 231.　　　　　　[8] Id. 278-280.

tensively intervenes between boulder clay in Lancashire, that arrangement is not structural but accidental, and he has recently published some sections of borings to corroborate this view. The same opinion is entertained by Mr. Rance, Mr. Harmer and myself respecting the sand and gravel beds in the purple clay of Yorkshire, and Mr. J. W. Judd, who has recently been at work in the counties of Lincoln, Rutland, and Leicester, also tells me that the sands and gravels in those counties pass horizontally into boulder clay, and he repudiates altogether any classification based upon the alternation of boulder clay with sand, while Mr. Wilson's description of the drift about Rugby [9] seems to suggest a similar conclusion." [1]

Speaking of the Eden valley, Mr. Goodchild says : "It is nearly impossible to make out any definite order of succession in the drifts in the lower parts of the valley; the few sections seen, show plainly enough that masses of sand and gravel pass into, and are interwoven with, clay drifts in such a way as to defy any attempt at separation over large areas, although single sections may be indicated which do show a definite sequence." [2]

In the Midland Counties drift composed of rounded pebbles and drift composed of angular flints, graduate into each other on the same horizon. There, also, drift interstratified with beds of sand containing sea shells may be seen on the same horizon with and graduating into drift in which no sea shells have yet been found. [3]

Again, Mr. Mackintosh says : "In the western part of the plain of East Denbighshire and Shropshire the upper boulder clay degenerates into a loamy gravel, which in many places covers the middle sand and gravel formation, as around Whittington, Oswestry, etc. South of Ellesmere it capriciously caps the large sand and gravel mounds (eskers?), covers portions of their sides, or lurks in the hollows between them, but the sharp line between the upper boulder clay and the underlying gravel or sand, so strikingly displayed all around the Irish Sea, is frequently absent." [4]

Mr. Bennett says : "It also seems to me that the glacial

[9] Quart. Journ. Geol. Soc. xxvi. 192.   [1] Geol. Mag. viii. 407.
[2] Q. J. G. S. xxxi. 81.   [3] Mackintosh, Geol. Mag. iii. 68, note
[4] Journ. Geol. Soc. xxxvi. 178-179.

drifts, so far as I have seen them, have been all formed at one period, and that not occupying so great a bulk of time as some would demand for them. Mr. Jukes Brown has reduced the drifts to an upper and lower division, and I should be induced to bring even these two into one. The drifts must have been more or less a tumultuous accumulation of deposits, with little or no order in their arrangement, being clay, gravel, and brick earth, according as the ice or water met with such materials as would make such deposits."

Mr. Bulman says: "The separation of the chalky clay and the purple clays in the eastern counties seems to have been made on very arbitrary grounds. They do not occur in the same district; the former occur on the west and the latter on the east of the Wolds." "In East Lincolnshire," says Mr. Jukes Brown, "there are only three localities where the brown boulder clay comes in contact with the white boulder clay," and he concludes, after examining them, that the appearances at those places are not against the supposition that the brown clays *pass into* chalky clay. "If the purple clay was subsequent to that of the chalky," says Mr. Bulman, "why was none of the former laid down on the latter over all that region west of the chalk wolds where it occurs? while at the same time it *does* occur further west and north away from the chalky boulder clay, and why, on the other hand, is not the chalky clay found beneath the purple on the east of the Wolds?" [5]

As regards the division between the purple and Hessle clays, Mr. Jukes Brown shows that the sands and gravels separating them graduate into both lower and upper clays; the sand beds contain patches of boulder clay, and the latter beds of sand in the usual way. He concludes, finally, that there is no break between the clays. [6]

If we turn from Europe to America, we shall find that the same conclusions have been arrived at there.

In his work on "Lake Superior," published in 1850, Agassiz says: "I maintain that the cause which has transported these boulders in the American continent must have acted simultaneously over the whole ground which these boulders cover, as they present throughout the continent an uninterrupted sheet of loose materials, of the same general nature, connected

[5] Geol. Mag. Aug. 1891, 345.   [6] Id. 345.

in the same general manner, and evidentially dispersed at the same time. Moreover, there is no ground at present to doubt the simultaneous dispersion of the erratics over Northern Europe and Northern America. So that the cause which transported them, whatever it may be, must have acted simultaneously over the whole tract of land west of the Ural Mountains, and east of the Rocky Mountains . . . . that is to say, at the same time, over a space embracing two hundred degrees of longitude." [7]

Again, Dana, after noting that the Americans have recognized three separate and distinct periods for the drift, goes on to say : "This generalization has been principally founded on the characters of the drift of Lake Champlain and that of the valley of the St. Lawrence . . . . but before we can adopt these subdivisions of the glacial period with reference to so many distinct modes of action of the transporting agencies, or of the different degrees of intensity with which they acted, it will be necessary to prove the succession of beds in question to be *general* and not merely *local.* . . . I see no reason in local facts of this kind to infer that there were three distinct periods with reference to the intensity or mode of action of the dispersing forces. Dr. Bigsby detected no evidence of this subdivision of the drift in the region which he examined further to the west."

Again, speaking of the rapidity with which the beds were deposited, Professor Dana says, " It may be queried, considering how much appears to have been done by a single wave, whether one year, or even less, would not have sufficed for the *upper* division or the upper twenty feet in certain parts of the formation. . . . . The deposition of a large part of the older ' alluvium,' if the above view is right, was a rapid work, much more rapid than has hitherto been suspected. . . . The evidences of rapid deposition are so many and obvious that they appear to set aside any theory of the glacial cold which demands a slow decline of the era. . . . On the west side of Hallechs' place the beds . . . evince the same free supply of material and rapid deposition under the action of the waves." Professor Dana then goes on to argue that since the beds over certain regions are sandy throughout and free from upper

[7] Op. cit. chapter x.

layers of fine river or bog detritus, such as is deposited about existing mud flats and sand *banks, that it appears quite positive these sandy beds did not lie for a long period beneath the water after the material was deposited.*

The facts and opinions I have just cited, attested by some of the most experienced explorers of the drift beds, seem to me to be conclusive against separating them into different horizons, and in favour of treating them as of one period, and, as it seems to me, distributed and arranged by the manifold operations of one versatile impulse, which could be no other than a mighty flood of waters which was alone capable of sifting them and then laying them down in this fashion.

Let us now turn to the other phenomena of the drift, and see how far they testify to the same conclusion.

The first fact which strikes an observer in travelling over wide districts occupied by the surface loose beds, is their continuous character. On wide plains, and generally when broken ground does not intervene to break them, they are found spread out in continuous sheets, with unbroken character sometimes stretching for hundreds of miles, with the rolling surface and contour which a great moving mass of water only can have given them. Sometimes, in estuaries and near the sea where the water was naturally agitated, they are aggregated into lenticular mounds or drumlins, or into kames and eskers, with whaleback curves to their surfaces, all marking the smoothing delicate handiwork of soft-fingered water, otherwise their contour is unbroken.

The external outline of the areas covered with drift in an open flat country like Russia or America points the same moral. Dr. Wright says of the latter : " When once the limit of the deposits containing striated stones and transported materials had been accurately determined, it was found that the margin was exceedingly irregular in two respects. The southern edge of this deposit is both *serrate* and *crenate*—that is, it does not follow a straight east and west line, but in places withdraws to the north, and, in others, extends in lobe-shaped projections far to the south. This constitutes its serrate character. But it is the crenate character of its southern border which is of most significance. The southern border

with its indentations and projections, is not determined by any natural barrier." I confess it seems to me that this feature, instead of pointing, as Dr. Wright argues, to a widespread viscous flow of ice, points to a widespread flow of water, whose irregular loss of momentum, due to local irregularities in the surface over which it flowed, would cause it to flow further along certain lines than others.

The same widespread flood seems to me to be absolutely necessary, if we are to explain the way in which the mantle of drift is not only spread out over the level plains, but also covers much of the broken country with its continuous embrace, irrespective altogether of the drainage or contour. It occurs sometimes up to great heights on the slopes of the hills, either in a continuous sheet or in detached masses where it was arrested by some projection or filled up some hollow, or, in many cases, ranged in terraces along the flanks of the hills, marking its high-water mark just like the long bank of shingle on the beach after high tides similarly marks the level of the water. The fact that these terraces, as at Glen Roy, are not horizontal, while conclusive against their having been deposited during a continuous submergence, are the best evidence of the movement of a great mass of water whose margin would rise as the valley up which it forced itself gradually narrowed and thus got throttled, as is the case with the so-called "Bore," at Cutch, in Northern India, and in the Bristol Channel, or the great upflowing tide from the Bay of Biscay at St. Malo.

Mr. L. Lyell, in referring to the terraces at Glen Roy which are not level, says, in fact, that they could only have resulted from great tidal action as in the Lofoden Islands.

Apart from these sloping terraces it has been noticed that the deposits with water-rolled-gravel are at different levels in different places, and there have been some ingenious devices to try and explain why the supposed evidence of submergence in Scotland should be limited to places below 700 feet in elevation, while at Moel Tryfaen, and elsewhere, they should require a submergence of twice this amount. While the fact remains inexplicable on other theories, it is most plainly explained by a great flood of waters whose normal level would be greatly raised wherever the water was squeezed into a narrower space, as, for instance, in running down St. George's Channel.

This again accounts for the mixing of marine and fresh-water shells, and of the bones of seals and whales with those of mammoths and mastodons, where we have no evidence of prolonged submergence, and where the heterogeneous mixture seems inexplicable on any other theory. I refer notably to the deposits of North Germany, and some of those in Eastern England and the West of Scotland.

The same cause would explain why the drift beds are so universally barren in the interior of the country, while they contain marine shells and other *débris* as we approach the maritime districts, and this not in one locality, but in Scandinavia, North Germany, Great Britain, and America. A general and continuous submergence of the country cannot explain this fact; a transient flood of waters does explain it, for in passing from the sea on to the land it would deposit the marine organisms it bore along on the first opportunity, and having sifted them out, would pass on to arrange and mix together whatever soft materials it encountered. This would also explain the mixture of shells from different depths in great banks, such as we find at Uddevalla, lying many feet thick, unbroken and cleared of sand and mud. Assuredly nothing but rushing water, whose gentleness in carrying fragile objects without breaking them, even when moving at a tremendous rate, has been often remarked and will be referred to in the account of the Holmforth flood to be presently described, can explain all this. Certainly no diurnal causes known to me would heap up such banks of shells as we find at Uddevalla and elsewhere.

Nor again can be understood by any such cause the mixture of *débris* from different localities in the drifts of many districts. We have already seen how difficult it is to account for such a mixture on any theory of land-ice, and especially when we discard the transcendental instrument known as a ground moraine. The ordinary operations of the sea seem equally impotent. No doubt along its margin which forms the beach a mixture takes place as the pebbles and stones are driven along by waves, but it is a mistake to suppose that these waves have any but a very local effect. In the deeper water they have, so far as we know, no effect at all, and there is consequently little or no transport of materials on the

sea-bottom by the water. A good proof of this is the unrolled, unweathered and sharp-edged mammoth bones covered with serpulæ, etc., which are dredged from the bottom of the North Sea.

While we cannot appeal to the sea or to other diurnal effects of water, we are compelled, it seems to me, to have recourse to a rapid and large flow of water, if we are to explain the mixture of materials from different neighbourhoods, as, for instance, of pebbles from Cumberland and the South of Scotland with shells from the Irish Sea at Moel Tryfaen.

Water moving in this way would sweep up and mix together and then throw down in great heaps when its current was arrested, all the *débris* that came in its way, and it must have been no ordinary marine submergence, but a widespread wave of waters which would pass over the country mixing the materials it met with. In the case of such a flood again, we should have the materials of the loose covering dominated very largely by the nature of the subjacent strata over wide districts. In Eastern England and Northern Germany, where there are no projecting rocks above the ground, whatever force distributed the soft beds, must have picked up in its passage the disintegrated surface *débris*, and mixed it together in a way which only a mass of water would be competent to pick up and distribute. This again accounts, as it seems no other theory can account, for the way in which the beds change their texture ; thus as we approach the chalk districts, the boulders and pieces of chalk in the clay become more and more conspicuous, whence its name of the chalky boulder clay. "When we get to the Lincolnshire wolds and all the district about Brandon, the chalk predominates so much that the clay becomes almost white, and at Mareham le Fen it is so fine as to be actually burnt for lime, while at Stretford it is exceedingly difficult to discriminate between it and the solid chalk. Similarly the boulder clay overlying Kemmeridge clay, which last is easily recognized by its dark-blue colour, is very largely composed of it, and taking two places far removed from each other as Woodhall Spa and March, a careful examination will alone show that it is a glacial deposit at all. So, too, with the Oxford clay, which imposes its light blue character upon the boulder clay where-

ever we meet with its outcrop." "Gault also takes the ground in but a small area of the Fens, but the boulder clay ' picks it out,' as it were, and at Modney Bridge brickyard, near Kelgay, for instance, I have shown," says Mr. Skertchley, " the glacial bed to be mistaken for gault by persons quite familiar with the latter. In like manner," he continues, " I might cite the numerous formations over which I have mapped this interesting deposit, and show how, in every case, the boulder clay is made up of the wreck of the subjacent rocks." This selective affinity of the boulder clay seems to me consistent only with the distributer of the clays having been a powerful and transitory wave.

This cause again seems to exactly explain the great contortions and swirling curves of the contorted drift as we meet with it in Norfolk, in the island of Moen in Denmark, and elsewhere, where we have most ample evidence, not only of water, which can alone account for the laminæ and stratification, but of water in the most violent movement, and moving, too, in a transient fashion. These vast curves, many feet in diameter, would have been broken up by repeated waves, and point to one such movement only ; a magnificent section many feet thick at Swinton, in Lancashire, close to my own house, is perhaps the finest example I know, both of the continuity of such curves, and of their vast sweep. The various phases of such a flood are marked also, it seems to me, by these contorted drifts being often deposited over beds lying in horizontal layers which have not been disturbed, a condition of things neither consistent with any action of ice, nor with the long-continued erosive action due to marine submergence.

Thus also we would explain the false-bedding of many of the drifts, and the oblique stratification of others. De Luc thus refers to the inclination of various beds of sand and gravel he met with: " In making the road to the bridge of Caroluge, they were found inclined at 30°. Between Plan les Ouates and Saint Julien in a pit they were inclined at 25° towards the north-west, near the village of Vervier at 15° towards the north-west, and on them rested others inclined at 8° to 10° ; at the Creux of Champel were beds of sands 10° in inclination. Near Nyon were beds of sand and gravel inclined at 22°. Near the

village of Corsan were several parallel layers inclined to the south-west at an angle of 25°. Near La Roche were beds inclined to the north at 22°, and immediately adjoining were others at 35°. These beds of gravel," he adds, "which were of limited extent, prove that the currents depositing them had not always a horizontal movement, but were subject to vicissitudes, and had currents partially inclined."[s]

Professor Dana, *inter alia*, says, " The stratification of the stratified drift is almost in all parts, except where too coarsely stony for it, of the *flow and plunge* style. . . . The oblique lamination indicates a violent onward movement in the waters ; and the division of the layers into parts proves that there was a heavy plunging in connection with the rapid flow. A single obliquely laminated layer is often one or two feet in thickness, and in one case observed it was eight feet, and each must have been formed at a single onward movement of the plunging waters. This structure of the stratified drift shows that water and earth were let loose at the time in immense quantities, and that consequently there was a very rapid piling up of the layers of gravel and sand."

In order to explain the dip in the laminations, Professor Dana also postulates an extraordinary change in the volume and violence of the waters (which, in his view, were prodigious rivers), by which the currents became so powerful as to overcome the action of the incoming tide, and took charge themselves of the deposition of the earth and gravel. In other words, " an extraordinary flood set in, extraordinary even at a time when a violent flood had been already long in progress, and the flood waters did the deposition even in the face of the tide."

As further evidence of this flood, he cites the character of the gravelly and stony deposits, of which he says, *inter alia*, " Such a transition in the drift deposits from the production of sand-beds with but little fine gravel to that of beds of coarse gravel and large stones and partly cobble-stones, proves that there was an equivalent change in the flow of the waters. These waters were in rapid plunging flow when the lower stratum was deposited . . . but, however great the previous violence, there was an increase afterwards to a vaster flood." He also quotes as further evidence the widespread erosion of drift

[s] Mems. de la Société de Phys. etc., de Genève, v. 100-101.

deposits as caused only by the tearing waters . . . the fiercely flowing waters often denuded the surface deeply and left the larger stones at one or more lower levels. The stony strata are found commonly at the top of the upper terraces, showing there was an increase of flow at the last." He urges that there is no cause to explain the various beds of gravel and cobbles " apart from a flood of enormous extent sweeping down the valleys," and inasmuch as river valleys are very widely apart over New England, and so many of them bear evidence of a flood, we may reasonably conclude that all New England was sooner or later swept by it. . . . The flood appears also to have risen rapidly to its height, for (1) the transition from the finer deposits to the upper coarser stratum is generally rather abrupt, and (2) at the lower extremity of the Quinnspear valley the change in the structure of the beds, from tide mark to flood mark, marked in the reversal of the oblique lamination, took place along a well-defined plane without any alternations, and was visible also in the colour of the deposited sands."

In another paper Dana adduces evidence of dominating currents of water having caused other phenomena of the drift; thus he describes " how the sand, pebbles, and cobblestones were thrown down together, but they underwent as they fell an arrangement which varied according to the movements in the waters beneath. The bay had its tidal currents as now, its areas of comparatively still waters, and besides certain channels along which the flow of the river increased greatly the force of the ebbing tide. The stratification of the deposits varied accordingly; when the currents were strong they washed away the sand from the stones, or if very strong the sand and smaller pebbles, and thus layers of coarse gravel were made—gravel beds being always deposits from which the sand has been sifted out by moving or flowing water. Along the main river courses there ought to be found, consequently, long *gravel courses*, marking the direction of the strongest currents, and these gravel courses should be not far below the surface, unless the depth of water in which they were deposited were too great for this." He then goes on to point out the existence in the district of several of these courses, and he adds, " The extent of the region shows that the flow producing it had the breadth and character of a tidal

flow." He also points out how sometimes the terraces on one side of a valley are considerably higher than on the other, showing where the currents flowed most rapidly, and, after quoting several instances, he says " it is evident that the central part of the great tidal wave up the bay in the Champlain era swept northwards over an area 1½ miles wide, but as it approached the south point of the East Rock range, it was pushed to the westward. . . . By studying the stratification we may even in some cases distinguish the effects of individual waves and changes in the action of tidal or river currents," and he concludes that " Mill river valley, especially its western side, was the course of a powerful tidal current. From the diminution in the amount of pebbles to the east of the river it would appear that it rapidly lost its force as it flowed in that direction ; and finally, as the beds of fine clayey sand show, there were intervals of comparative quiet, or of only gentle movements. This is proved, not only by the fineness of the beds, but by the very delicate contorted lamination in them, which in some places looks as if due to the smallest of eddyings in the waters at the time of deposition, and also by successions of obliquely laminated layers of sand only one or two inches thick, constituting here and there an overlying bed. When layers of stones, or thick obliquely laminated sand beds, exist between these clayey beds, they indicate that a time of rougher movements intervened." He also mentions the change of currents evidenced by the varying slope of the laminations, some sloping to the south and others to the north, and, after discussing the various possible causes, he concludes in favour of a flood, " a flood enormous in extent and vast in effects."

The kames, eskers, äsar, etc., so far as the best judges agree, are the result of the meeting of conflicting currents and masses of water, the volume of which caused a heterogeneous deposit to prevail very largely throughout them, which was succeeded as the waters became more tranquil by a stratification near the top. Such a flood of waters as I have mentioned, flowing for the most part in one direction, would account for their general parallelism, and being transient would account for the regularity of their internal structure and forms.

Such a flood, when it entered a strait at both ends or a crooked and contorted channel, would break into or would

meet other waves, and the place where the collision occurred would, as we have seen, be where the kames and eskers would naturally be found, while it is probable that the breakdown of the earth's crust which, in my view, caused the floods would cause several waves coming from different points, whose collision would have the same effect.

Such a mighty flow of waters would alone, as it seems to me, leave those great tails of *débris* headed by a mountain or some boss of rock, which are so frequent in Scotland, and are there known as examples of crag and tail formations. The known action of the sea would have swept them away and levelled everything down, and only a transient mass of water moving along would, it seems to me, have had this effect.

Such a flood again would account for the variation in the depth of the deposits in many areas in close contiguity, piling them up like drifted snow is piled up, where a sheltered nook or some obstacle prevented the moving water from having its proper denuding effect and throwing them aside and sweeping them away where the current was very impetuous.

The enormous depth of the drift in certain places is not generally recognized. Professor Geikie tells us how, in the valley of the Clyde, the drift deposits in one place reach the excessive thickness of 355 feet.[1] In Germany Wahnshaffe mentions many places where it exceeds 100 metres, and one where it reaches 171 metres in thickness.

The same vast depths of deposit of one similar texture and, so far as can be seen, deposited by one and the same tremendous effort, are found in Holland, where they seem to have been poured into huge chasms caused by the synchronous subsidence of the surface. In some places in America they have also reached a similar portentous thickness, as may be seen in Dr. Wright's work.

We find the same great depths of drift sometimes at the heads of our English valleys, where the flood of water would carry its load and then be forced to deposit it by the obstructing mountains at the heads of the valleys. By no other force can we understand how this mass of *débris* of sifted sand intercalated with clay and gravel should have been forced up so many of our valleys contrary to their slope and

[1] The Great Ice Age, 185.

contrary to the direction of their streams. Neither ice nor the diurnal effect of water could have done this. Nor again could either of them have left great masses of rolled gravel and sand high up on the Highland mountains, sometimes on the very saddles and cols.

It is not merely the depositing power of a flood of water which we have to consider. Quite as remarkable is its denuding power. In many districts, and especially where if there was a flood it naturally flowed with great speed and violence the country has been stripped of *débris* and the bare rock has been swept clean, while the *débris* has been driven into the hollows and clefts. This is notably the case in such places as the Little Orme's Head, where the double operation can be well seen. In high latitudes, men were struck from early times by the occurrence of great masses of boulders lying detached and uncovered. *Inter alios* such boulders attracted the notice of Hearne in North America, and the early explorers of Finland and Sweden. North of Labrador they are described as rammed against the hill like a pavement. They occur in a denuded and uncovered fashion over wide areas.

In Finland, Smaland, Labrador, and the barren lands of America, such boulders are piled up on the top of each other with no sandy or loamy matrix, as if all the softer materials had been washed away by a mighty flood, and this view is strengthened when we find that as we travel southwards the blocks diminish in number, and the quantity of sand and loam increases until in a large area on the continent the blocks disappear altogether, and we have enormous deposits of sand known as Diluvian-Zand, or Sable Campinien, occupying the greater part of Dutch and Belgian lowlands. It would be natural if there was a great flood that the softest and finest of the materials which the water swept away should be carried the furthest and left in wide-spreading layers; but this is quite inconsistent with any action of land ice, or of the diurnal operation of water, and, in fact, of any other agency than that of a great flood.

There can be no doubt whatever, that water, if it moved the blocks at all in the way we find them distributed, must have done so when moving in a cataclysmic fashion, and both with

great volume and with great rapidity. This was seen by the older writers, who nevertheless failed to analyze the conditions of the problem.

The fact that so many of the boulders are rounded, and that many of those which are not have their edges and angles blunted, shows that they have been more or less rolled and carried by water. This is generally admitted. Another obvious fact was tersely stated by Mr. Hopkins, namely, that " a force which is capable of rolling a stone is also capable of transporting it, and inasmuch as most of the stones must have been rolled by water where their edges are rubbed off, it follows that water also transported them." There has been great hesitation, however, in attributing to water the portage of such vast masses, and it has been continually said that it is incapable of doing the work. This seems to me quite just if we limit the operations of water to its diurnal action only.

It was Scott Russell who, in his famous report upon Waves, published in the Transactions of the British Association, first analyzed the subject of the abnormal action of rapidly flowing water, and was the first to discriminate the existence of a great primary wave of fluid, differing in its origin, its phenomena and its laws, from the undulatory and oscillatory waves which had alone been up to that time examined. This wave he called a great primary wave of translation. Its velocity in channels of uniform depth is independent of the breadth of the fluid, and equal to the velocity of a heavy body falling freely by gravity through a height equal to half the depth of the fluid, reckoned from the top of the wave to the bottom of the channel. Its velocity is not affected by the velocity of impulse with which the wave has been originally generated, nor do its form or velocity appear to be derived in any way from the form of the generating body. By its transit the particles of the fluid are actually raised from their places, transferred forwards in the direction of the motion of the wave and permanently deposited at rest in a new place at a considerable distance from their original position. There is no retrogradation, no oscillation; the motion is all in the same direction, and the extent of the transference is equal throughout the whole depth. . . . The velocity of such a wave is that due to gravity acting through a height equal to the depth

nel below the surface of the fluid. The height of the wave may be indefinitely increased by propagation into a channel which becomes narrower in the form of a wedge, the increased height being nearly in the reverse ratio of the square root of the breadth.[2]

Elsewhere Mr. Scott Russell defines a wave of translation as solitary progressive, and depending chiefly on its depth.[3] In a subsequent report, he entered at greater length into a discussion of these waves, which, he says, furnish a type of that great oceanic wave which twice a day brings to our shores the waters of the tide. On the other hand, the waves of the sea only become waves of translation after breaking on the margin of a shoal and rolling along in shallow water towards the beach until they finally break on the shore. In regard to the absolute motion of translation, Mr. Russell says " the total effect of transmitting a wave of the first order along a channel is to have moved successively every particle in the whole channel forward, through a space equal to the volume of the wave divided by the waterway of the channel. . . . The horizontal range of motion of the wave is necessarily determined by the volume of the wave, while the length of the wave is dependent on the depth of the water through which it flows."

One of the most important features about such waves is, as Mr. Russell has shown, their being vehicles for the transmission of mechanical force. " The wave," he says, " is a receptacle of moving power, of the power required to raise a given volume of water from its place in the channel to its place in the wave, and is ready to transmit that power through any distance along that channel with great velocity, and to replace it at the end of its path." [4]

The discovery of Mr. Scott Russell was soon applied to the explanation of geological facts. In his famous memoir on the " Transport of Erratic Blocks," published in the Transactions of the Cambridge Philosophical Society, vol. viii., Mr. Hopkins investigated the transporting powers of currents of water, and showed that under certain conditions currents of water would be generated of sufficient velocity for the transport of boulders,

[2] Report Brit. Assoc. vol. vii. 423-425.   [3] Id. xii. 19.
[4] Id. xiv. 311-390.

and that consequently this cause is also adequate to produce the removal of at least a large portion of the boulders which have travelled from their original sites, and that the theory is therefore not to be rejected on account of any apparent inefficiency in the cause of transport assigned to it, or the extravagances which have formerly been associated with it.[5] After dividing currents of water into river currents, tidal currents, ocean currents, and elevation currents, he discards the first three, and limits his examination to the last only, which are always accompanied by a corresponding temporary elevation of the water constituting a wave. He bases his conclusions on two data which had been established by Mr. Scott Russell: I. " Every particle in the same transverse section of the canal in which the water moves has the same motion. II. The velocity with which the wave is propagated is equal to that due to half the height of the crest or highest point of the wave above the bottom of the canal."

He goes on to show that such waves would be produced in the ocean by the elevation of a portion of its bottom. The height and breadth of the wave would depend on the area of the elevated portion, the height through which it is raised and the time occupied in its elevation.

Suppose the area were circular and the elevation instantaneous, the wave would diverge from it as a centre and would have a steep front like the tidal wave, called *a bore.*

" If the elevated area were a parallelogram, of which the length was much greater than the breadth, two waves would proceed in directions perpendicular to the longer sides of the area to which side the fronts of the wave (except near its extremities) would be parallel. The breadth of the wave would depend on that of the elevated area. The diminution in the height of the wave and consequently in the velocity of the resulting current would be much less than in the case of a circular wave. Instances of circular waves would necessarily present themselves in the elevatory movements of such a district as that of the Cambrian mountains, while wholly or partially beneath the sea ; and examples of the other kind, in the simultaneous elevation of the whole of such

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[5] Op. cit. 220.

a range as the great mountain limestone ridge of the northern part of this kingdom. . . . If a portion of a great wave pass into the mouth of a channel which gradually contracts, the velocity of the attending current might become much greater than in the uncontracted wave. Such must have been the case with respect to the portion of a wave diverging from the district of the Cambrian mountains, and received into the strait which must have been formed by the pass of Stainmoor." [1]

Turning from these general considerations to the actual transporting power of currents, Mr. Hopkins says : " Whatever be the specific gravity of a body, if its dimensions be sufficiently small, it can never acquire more than a small velocity in descending by gravity in any fluid of which the density is not extremely small. Such a body may therefore be transported to considerable distances before it descends to the bottom, or when once deposited on the bed of the stream it may easily be again disturbed, and carried onward as before. When the body is not, however, of very small dimensions, it can only be transported along the bottom by the impelling force of the current, its motion being retarded by friction or the resistance of solid obstacles. In this latter case it is necessary to ascertain the relation between the velocity of the current and the dimensions and weight of the largest mass it is capable of moving. Their relation depends not only on the volume and specific gravity of the mass, but also on its form."

Again, " A body acted on by a current may be moved by sliding or rolling. In the former case, a very uncertain element, the friction of the surface on which the body rests, is necessarily introduced into our calculations. It will depend on the nature of the surface on which it is moving, and the force with which the body presses on that surface, and on whether it presses evenly all over, or water can get underneath it so as to raise the body up. Where the body moves by rolling, the motion is independent of the magnitude of the friction. And if a body begins to roll and we would calculate the force of the current just sufficient to keep it in motion, we may consider the fluid pressures as acting on every part

[1] Id. 224-5.

of its surface, and the result will be approximately true whatever the nature of the surface over which it moves, providing the surface be sufficiently firm to give the requisite support to the rolling body. The force, however, thus determined, might be insufficient to make the body begin to move, since it might rest in such a position as to exclude the fluid action from its lower surface. In many cases the incipient motion might be due to accidental causes, as, for example, an impulsive blow from another mass already in motion ; moreover it is probable that all blocks which may have been transported to considerable distances by this agency have been carried on by currents of considerably greater force than that just sufficient to keep them in motion, and which may have been sufficient without accidental causes to move them from rest even under conditions not the most favourable for their movement." [2]

Mr. Hopkins then goes on to examine the conditions of the movement of blocks of various shapes. *Inter alia* he concludes that we should get an exceedingly erroneous result of the transporting power of rapid currents if we based our opinion on ordinary currents subjected to our observation. Thus, if a stream of ten miles an hour would just move a block of a certain form of five tons weight, a current of fifteen miles an hour would move a block of similar form of upwards of fifty-five tons, and a current of twenty miles an hour would, according to the same law, move a block of 320 tons. Again, according to the same law, a current of two miles an hour would move a pebble of similar form of only a few ounces in weight, and it should also be remarked that minute inequalities, or a want of perfect hardness in the bed of a current which would produce little effect on the motion of a large block, would entirely destroy that of a small pebble ; so that the circumstance of the transporting power of a stream of two or three miles an hour being inappreciable is perfectly consistent with the enormous power of rapid currents.[3]

In regard to the impediments to such motion, Mr. Hopkins says, if the surface of the ground were soft like that of clay it would be unfavourable, while the want of cohesion of a sandy bottom from its opposing a less effective resistance to a

[2] Op. cit. 225-6.   [3] Id. 233.

motion rather by sliding than rolling, might be highly favourable. Abrupt inequalities such as those presented by ravines or steep escarpments would present insuperable impediments, but regular ascents, without rugged inequalities of surface, would offer no such serious impediments.[4]

"It appears," says Mr. Hopkins, "by the table given, that a wave of between 50 or 100 feet in height in an ocean of the original depth there supposed, would be accompanied by a current of which the velocity would be from ten to twenty miles an hour;" and he demonstrates, that "currents of that velocity would possess a motive power abundantly sufficient to move the largest blocks, the transport of which it would be deemed necessary to refer to this cause." He particularly directs the attention of the reader to the fact that the space through which a block may be transported by a single wave is equal only to a small fraction of the breadth of the wave. Consequently a great number of waves might be necessary for the transport of blocks to distances to which they frequently have been transported. "It must also be recollected that sudden or paroxysmal elevations only will produce waves of elevation of considerable exporting power. Hence it follows that this theory of transport is essentially and necessarily associated with that theory which regards the phenomena of elevation as the consequence of a series of paroxysmal movements," the movements by which, in his opinion, those phenomena can be most satisfactorily accounted for. "The instantaneous elevation of a determinate portion of the bottom of the sea would produce a wave whose height would be equal to that of the elevation itself, so that it may be asserted in general terms that the theory of transport by elevation currents, in its application to existing phenomena of transport, involves the hypothesis of a succession of paroxysmal movements beneath the ocean, the height of many of which must have varied from 50 to 100 feet at least."[5]

Mr. Hopkins, writing in 1852, further says: "There are many persons who yet fail to realize in their own minds the enormous power which currents of water possess, even without greater velocities than may easily be allowed them. This

[4] Id. 237.   [5] Id. 238.

power arises from the fact, which I have elsewhere demonstrated, that the moving force of a current estimated by a block of any assigned form and material, increases as the *sixth power* of the velocity of the current. It is on this account that, reasoning from the power of ordinary currents of two or three miles an hour, we are liable to miscalculate so entirely the force of a rapid current. . . . It will not be denied by anyone, I imagine, that it would generally be the *necessary* consequence of a transitory current driving a mass of drift over a level surface, to spread it out in an approximately equable layer; while such a result could generally be regarded as only the *accidental* consequence of transport by floating ice. Such a layer would indicate the latter as a *possible* mode of deposition, the former as a highly *probable* one. When the glacialist contends for the *possible* rather than the *probable* mode, let him examine himself strictly whether he may not be unconsciously under the dominion of preconceived theoretical views."

In a paper on "The Geology of the Lake District," the same writer says: " Paroxysmal elevations, beneath the sea, varying from 50 to 100 feet in height, may produce currents of which the velocities shall vary from at least five or six to twenty miles an hour, provided the depth of the sea do not exceed 800 or 1000 feet." [6]

Again he says, "Suppose the form of a block as nearly spherical as many erratic blocks are observed to be, there is no doubt that blocks of five tons and upwards might be moved by a current of ten miles an hour; and assuming the force of the current to increase as the square of the velocity for greater velocities than that, it follows that a current of fifteen miles an hour would move blocks of similar forms of the weight of fifty-six tons and upwards; while a current of twenty miles an hour would move similar blocks of 320 tons and upwards. For other forms the weights might be much less; but these calculations demonstrate beyond doubt, that while an ordinary stream of between two and three miles an hour may be insufficient to move a pebble, a current of from ten to twenty miles an hour may have motive power sufficient to transport blocks of enormous magnitude.[7] . . . Nor does

[6] Q. J. G. S. iv. 92.   [7] Id. 92-3.

this view of the subject require the hypothesis of paroxysmal elevations of great magnitude; for it appears from what has been stated, that an elevation of 100 or 150 feet would produce a current capable of transporting, for at least a short distance, and under favourable conditions, a block of immense weight." [8]

In 1847 Dr. Whewell discussed before the Geological Society the question of the efficacy of waves of translation to account for the Drift. Basing his calculation on the hypothesis that the Northern Drift occupies a space 2000 miles long, and 400 to 800 miles wide, or roughly a semi-circle with 800 miles radius, supposing again all the drift to be derived from one centre, he takes an inner circle of 200 miles radius, and he considers the drift as occupying only the annular space between these two circles, the mean distance from the centre of the annulus being 500 miles. He supposes that at this mean distance every square mile contains as much drift as would cover it entirely to the depth of a hundredth part of a foot, which is equivalent to supposing there is a patch of drift one-tenth of a mile square and one foot deep on each square mile, or a ridge or trainée of drift one-tenth of a mile long, one-hundredth of a mile broad, and ten feet deep. He supposes again the specific gravity of this material to be three times that of water. When the materials were immersed in water the effective gravity would then be twice that of water.

" The horizontal force required to move a body along a surface on which it rests, depends on the form of the body, its texture, and that of the surface and other circumstances, but we may suppose it would require a force and pressure of at least one-fourth the weight of the mass moved to propel rocks and loose materials along the bottom of the sea; that is to say, it would require a pressure equal to the weight of half a cubic foot of water to move a cubic foot of drift, and so for other quantities."

" As the mass of drift has travelled on an average 500 miles from the centre, and the labouring force which has carried it through this space, in whatever way it has acted must be equivalent to the product of the moving pressure and the

[8] Id. 93.

space through which it has acted, and taking the data above given, it roughly means that a cubic mile of water rising through two and a half feet would supply the power necessary to carry the drift which occupies one average mile at the mean distance from the centre of distribution, or a space of ten square miles one-hundredth of a mile in depth, rising through the same distance, would have the same effect, and the further conclusion follows that 450 cubic miles of water, raised a mile high, would produce an effect equivalent to the dispersion of the whole body of Northern Drift. This is equivalent to 4500 cubic miles of water raised through a space of one-tenth of a mile; or again, to a body of water 45,000 miles in surface, and one-tenth of a mile deep, raised through one-tenth of a mile. If then we suppose a sea-bottom 450 miles long and one hundred miles broad, which is one-tenth of a mile below the surface of the water, to be raised to the surface by paroxysmal action, we shall have the force which we require for the distribution of the Northern Drift, on the numerical assumptions which have been made. And this is true, whether we suppose the elevation to have taken place at once, or by repeated operations, so long as they are paroxysmal. We shall have the requisite force, for instance, if we suppose this area to be elevated by ten jerks of fifty feet each, fifty jerks of ten feet each, or by the same 500 feet anyhow divided into sudden movements. And as we diminish the area elevated, we must increase the total amount of elevation in the same proportion; so as to retain the same ultimate product of water paroxysmally elevated through a certain space. In all these cases, we shall have a machinery which, operating through waves of translation, will produce the requisite effect. And if any of our data be held to be erroneous: the area occupied; the amount of matter in the drift; the amount of friction or tenacity to be overcome in propelling it, or the law of its diminution in quantity as we recede from the centre of distribution; the result will have to be proportionately diminished or augmented."

Dr. Whewell then goes on to show that no gradual and gentle elevation would suffice, it must be paroxysmal; and he concludes, " The great mass of northern drift, inasmuch as no considerable part of its transfer can be accounted for by any minute causes or languid operations of water, is an irresistible

evidence of paroxysmal action; and of action on a scale which may be judged of from the conclusion at which we have arrived, an elevation of 45,000 square miles of sea-bottom through 500 feet, and this conclusion is equally certain, whether or not we consider the machinery employed in the distribution of this mass from the centre to be waves of translation. For the proposition that the labouring force expended in the transit of this mass of materials must be equal to the force exerted, and that this force must be exerted in such portions as at every step to overcome the friction and tenacity of the masses of rock, shingle and other detritus moved, is equally true whatever be the machinery employed. As no gradual or minute action could move the masses in question through a yard of space, no accumulation of such action, through any amount of time, could distribute the masses through the great distances which the northern drift has traversed, and spread them over the vast spaces which that formation occupies. The distribution of the northern drift belongs to a period when other causes operated than those which are now in action." [9]

Whewell further says, that "a wave of translation differs from a debacle, which was conceived of as a vast torrent sweeping over the land arising from the emergence of a submarine area; and that it is, in fact, *a debacle travelling along the sea* after it has been shot off the land." [1]

Finally, Rankine calculated the speed which a current must have had, in order to carry along *débris* of a certain size :

Soft clay requires a velocity of	. .	0·25 feet per second.
Fine sand	.	0·50 ,, ,,
Gravel as large as French beans .	.	1·00 ,, ,,
Gravel of pebbles one inch in diameter	.	2·25 ,,
Larger blocks of rocks . . .	.	6·0 ,, and upwards.

All these facts and opinions of very competent men, who were mathematicians as well as geologists, seem to me conclusive that whether water moving in a tumultuous way did or did not move and scatter the boulders as we find them, it was powerful enough to do the work, if we can only postulate a sufficient impulse. Let us now turn from deductive reasoning to the inductive proofs, that such great stones have been moved

[9] Q. J. G. S. iii. 227-232. [1] Id. 232. [2] Prestwich, Geology, i. 83.

by water under abnormal impulse. In the instances which I shall quote, it will only of course be possible for me to refer to cases which have occurred within human experience, and not to such a catastrophe as the facts of the drift compel us to postulate.

In a communication to the Royal Society on the Earthquake of 1755, as seen at Cadiz by Mr. B. Bewicke, an eyewitness, he says : " Looking out to sea, we saw a wave coming at eight miles distance, which was at least sixty feet higher than common. Everybody began to tremble; the sentinels left their posts, as well they might; it came against the west part of the town, which is very rocky; the rocks abated a great deal of its force; at last it came upon the walls, and beat in the breastwork, *and carried pieces of eight or ten tons weight, forty or fifty yards from the walls.*" [3]

Don Antonio de Ulloa, describing the same occurrence two pages later, in the same volume, says : " The tides continued, with some intervals, till the evening, but lessening. They have ruined one hundred toises of the rampart, part of which, of three toises length and of their whole thickness, were carried by the torrent fifty paces." [4]

To turn to more every-day occurrences, a very graphic account of the famous flood caused by the Drance in 1818, in the Val de Bagnes, is given by Basil Hall, in his book entitled " Patchwork." He tells us how " the church presented one of its corners to the advancing tide, but although it escaped destruction, it was nearly half full of sand, mud and stones, brought there by the flood. The pulpit just peeped above the mass of rubbish, but the altar was no longer visible, being quite buried under the mud; . . . all the hedges, garden-walls, and other boundary lines and landmarks of every description were obliterated under one uniform mass of detritus, which had levelled all distinctions in a truly sweeping and democratic confusion. In every house, without exception, there lay a stratum of alluvial matter several feet in thickness, so deposited that passages were obliged to be cut through it along the streets, as we see roads cut in the snow after a storm. On the side of the building, facing up the valley, was collected a pile of large stones,

[3] Phil. Trans. xlix. 424.   [4] Id. 427.

under all these a layer of trees, with their tattered branches lying one way, and their roots the other. Next came a network of timber beams of houses, broken doors, fragments of mill-wheels, shafts of carts, handles of ploughs, and all the wreck and ruin of the numerous villages which the debacle had first torn to pieces, and then swept down the valley in one undistinguishable mass. . . . . From every house and behind every tree there extended down the valley a long *tail* or train of diluvial rubbish, deposited in the swirl, or, as a sailor would say, in the eddy, under the lee of these obstacles. All over the plain, large boulders or erratic blocks lay thickly strewed. These varied in size from a yard to a couple of yards in diameter ; but just at the point where the ravine of the Drance leaves the mountains, and joins the open valley of Martigny, I examined some enormously large masses of granite, which the inhabitants assured me had been brought down and placed there by the sheer force of the debacle. . . . . I well remember the awe and wonder with which I looked at one of the masses of rock pointed out to me, which the stream had evidently projected fairly out of the gorge into the plain. It measured 27 paces round, 12 feet in height, and 12 feet across in one direction, which I fixed upon as about the average. It was of a rude pyramidal shape. Further up the glen, I came to many rocks, which, though much larger than the one I mentioned, bore indubitable marks of having been in motion. This, however wonderful, seemed at first less difficult to imagine than it was to account for a number of comparatively small boulders, scattered over the plain, lying among the houses far from the gorge of the Drance. . . . Some of these rested either on the top of the deposits of sand and mud, or not far from their tops. It is probable, I think, that these masses may have been borne on the surface of masses of ice, or, more probably, included in them, and thus might float along till the diminished depth of the flood, caused by its spreading out, caused them to take the ground. On the melting of the ice, the stones resting upon them, or imbedded in them, would naturally rest on the surface." [5]

In a paper by Mr. Culley, he describes how in 1827 a rivulet in the eastern part of the Cheviots was so charged

[5] Patchwork, i. 33-36.

with water that it transported enormous accumulations of several thousand tons weight of gravel and sands to the banks of the Till, carried away a bridge then in process of building, some of the arch-stones of which, weighing from half to three-quarters of a ton each, were propelled two miles down the rivulet. It also tore away from the abutment of a mill dam a large block of greenstone porphyry, weighing nearly two tons, and transported it to a distance of a quarter of a mile. Instances were related as occurring repeatedly, in which from one to three thousand tons of gravel were removed to great distances in one day, and the author stated that "whenever 400 or 500 cartloads of the gravel were removed for the repair of the roads, one moderate flood replaced the loss with the same quantity of rounded *débris*." [6]

In 1852 Professor Prestwich read a paper before the Geological Society on the " Effects of the Holmfirth Flood," which was caused by the bursting of a large reservoir. He tells us " The weight of the materials swept away from the embankment, and scattered in gradually decreasing quantity for a distance of half a mile, could not have been much less than 40,000 or 50,000 tons. Where the valley was narrow, the water tore up the surface to a depth of 10 or 20 feet, and carried away large masses of rock to considerable distances. When the breadth of the valley increased from 30 to 50 yards to 100 or 200, the force of the flood abated. Still, the meadows were entirely covered to a depth of 1 or 2 feet with masses of rock mixed with sand and gravel. The bulk of the *débris* consisted of angular fragments not exceeding 1 or 2 feet in diameter, but among them a few large round blocks stood out in prominent relief. One of them measured 7 feet in extreme length by 5 in breadth and $2\frac{1}{2}$ in depth, and weighed probably from 5 to 6 tons, and was transported half a mile. Near it was another mass of almost the same size ; a third block rather nearer the reservoir measured 12 feet by $6\frac{1}{2}$ and 2 feet deep, and probably weighed 7 or 8 tons. The most remarkable block, however, lay in the middle of the valley near Upper Digley Mill, and at a distance of a third of a mile from the parent rock. It was 22 feet long, 6 feet broad and $3\frac{1}{2}$ thick, and probably weighed about 20 tons. Although the greater part

[6] Proceedings Geol. Soc. i. 149.

of the valley was covered with *débris*, there were places where its surface had been torn up to a depth of from 4 to 5 feet. . . . A stone post at the entrance to the churchyard was broken in two, and one part, 6½ feet long by 1½ square, was found 150 yards lower down the valley. All the slabs on the tombs were moved from 50 to 200 yards, and this occurred where the waters, having spread out a good deal, had lost much of their force. For 2½ miles down from Digley Mill were found pieces of a peculiar dark-grey micaceous flagstone, 6 to 12 inches in length and 2 to 3 feet thick, both in a rough state and squared for building, etc. The fields were covered with gravel and fragments of rock, none large, but some from 1 to 2 tons in weight . . . large iron boilers were carried considerable distances. That at Digley Mill, weighing 10 to 12 tons, was found more than a mile down the valley." One or two facts are interesting on wider grounds, thus: Dr. Prestwich remarks that, notwithstanding the force and violence of the torrent, fishes with which the reservoir was well stocked were transported a long way comparatively un-injured. Many were picked up in a state perfectly fit for use. He could find no shells or fragments of shells in the trans-ported gravel or sand, neither did he observe any in the bed of the reservoir. The fragments of rock showed little or no water wear. They were chipped, much broken, and angular. This is assuredly very interesting in view of some of the phenomena of the drift, such as the transport to long distances of angular stones and the occurrence of fragile shells in trans-ported gravel. Dr. Prestwich concludes his account in these words: "If such are the remarkable effects of a temporary flood, caused by a body of water comparatively so small, and along a valley where its power could not be maintained, we may form some conception of the enormous power which a more continuous flood, with more sustained action, would possess." [7]

In February, 1859, heavy floods or some other cause burst the banks of a succession of three lakes, which formed reser-voirs for the Crinan Canal, and were situated some 700 feet above it. This rush of water carried along with it stony *débris*, including boulders several tons in weight; the engineer who

superintended the repairs said he had removed several which were eleven tons in weight. The water had swept away the soft covering of stony earth or drift from the subjacent rocks, " carrying the boulders and stony *débris* before it, and throwing them down in those spots when the force of the torrent began to fail. " I saw many blocks," says Jamieson, " that it had borne along, measuring 3 to 4 feet in length, and a few even from 8 to 11 feet. The *débris* resembled some of the coarser kinds of gravel so often covering drift in river valleys." [8]

In a paper by Mr. Frederic Smith on the " Great Flood in the Devon Valley," which occurred on the 28th of August, 1877, he says, " A river 4 feet in depth was running as a branch from Dollar Burn down the railway cutting. The bridge had become blocked, and the water coming against the obstruction rose in the air as from a volcano, tossing· trees and boulders about like straw. . . . The water was perfectly thick with sediment and darkened colour, while every now and then mighty boulders were brought into collision with a noise like distant cannon. . . . I now reached the special scene of this disaster, viz., the washing down of the houses. These houses were of stone, and were swept away by being undermined. . . . I noticed the corner-stone of the houses hurry past me, and wondered where it would be carried. Later in the day, I found this stone about a third of a mile lower down, settled upon the railway; one of its diameters measured 3 feet and the other 2 feet 4 inches. Where the houses fell, an area 300 yards in length by 40 feet in average breadth and 6 feet in depth, was swept out. Here, then, at least 5000 or 6000 tons of material were removed, much of it being large stones and boulders, some of the latter weighing from 10 hundred-weight to a ton. . . . The water, laden with its boulders, must have been shot out of the glen as from the mouth of a cannon, for, instead of following the usual course, it went straight over its bank and deposited mighty boulders over a large area 12 feet at least above the present level of the burn. Several of these boulders were a little short of 3 tons in weight, and many weighed from 1 to 2 tons. Some of them alighted with such precision and force that they were shattered

[8] Q. J. G. S. xviii. 165.

into several fragments. In this mass there must have been some 6000 tons of *débris*. A little lower down the water again broke away at a tangent, and strewed a similar deposit over a large area, but the boulders were, on the whole, not quite so large. . . . As the water lost its transporting power, it filled up its own channel in its lower levels with pebbles and boulders to the extent in some places of 4 or 6 feet, so that the burn at the latter part of the day ran at a higher level than it had formerly done. . . . Coming once more to the railway, we found an immense heap of boulders, some being half a ton in weight, blocks of stone from the ruined houses, trees, and shattered furniture, with smaller stones and sand in abundance. From the surface of the railway that goes over the burn, 3000 tons were drawn away, and from the burn itself 3000 tons more, while a similar quantity has since been rolled down by the burn and taken out by the railway company. Here also a considerable piece of land was denuded . . . still lower down the water broke through an embankment and strewed an immense quantity of boulders in sand and clay upon several fields." "Since the paper was read," adds Mr. Smith, "Dollar Burn has reduced its bed to about its original level; in effecting this it has rolled not far short of 15,000 tons of stones, etc., down to the railway bridge. . . . A sunk wall has, since the flood, been completed on both sides of the burn, built almost entirely from the enormous accumulations described as having been formed at the mouth of the glen. This is altogether about a mile and a half long, and contains more than a ton for every yard." [9]

These results of a spate in a mountain stream in the Ochils, caused by a very rapid rainfall, are surely very eloquent of what would happen in the case of a great tidal wave. We are told the whole damage was done in little more than an hour, and the magnitude of the result is doubtless due very largely to the suddenness rather than the extent of the flood.

" Those," says Mr. Scrope, "who have watched the descent of floods in any mountain region, such as the Alps or Apennines, especially after violent local rains, or the fall of a water spout, will have observed that the torrent appears

[9] Trans. Edin. Geol. Soc. vol. iii. 169-178.

to be composed as much of stones as of water. Every pebble and boulder is in motion, all descending more or less rapidly to a lower level; all exerting an abrading force upon other stones, or on the banks of the stream. In the Vivarais, I have seen blocks of granite many tons in weight moved to considerable distances by torrents which are dry in summer. The large prisms of basalt, separated by the action of the frost, or an undermining stream from a cliff of this rock, lie for some time perhaps at its base, but are soon carried lower by a flood, and at the distance of a mile or two are seen to have been worn down to rounded blocks." [1]

Mr. Koch, in a discussion on a paper on Boulders by Mr. Mackintosh before the Geological Society, said that " in the late floods in Bohemia, blocks as large as six feet by four feet had been transported by the rivers." [2]

Mr. Tylor quotes a remarkable instance. He tells us " That in 1866, twenty inches of rain fell in Scinde in twenty-four hours, in a flat country intersected by rivers. Nine girders, weighing nearly eighty tons each, were washed off the piers by the Mulleer river from the railway bridge, situated sixteen miles above Kurrachee. . . . They fell in the course of six hours, and one girder of the weight of eighty tons was carried two miles down the river, and nearly buried in sand." [3]

Godwin Austen thus describes a flood in the Upper Himalayas : " While lying in my tent, after finishing up my work, I heard an unusual rumbling sound. . . . . Some Balti coolies shouted out that the stream was coming down. . . . . Before the black stream reached us it divided into two, and we then saw that it consisted of a mass of stones and thick mud, about thirty yards in breadth and about fifteen feet deep. . . . It was a most wonderful sight : a great moving mass of stones and rock, some of great size, measuring ten feet by six, all travelling along together like peas shot out of a bag, rumbling and tumbling one over the other, and causing the ground to shake. The large rocks lying in or near the edge of this moving mass would receive a few buffets, totter a little, and finally roll in among the rest to carry others away in turn. No one who has not seen a flood of this kind

[1] Geol. Mag. iii. 197.　　　　　　　　[2] Id. x. 185.
[3] Geol. Soc. xxiv. 124.

can form any idea of the mighty power of transport which the accumulated masses of water and melted snow acquire at these times, and I was almost bewildered by the spectacle." [4]

Blandford tells us that blocks three to four feet in diameter may be seen in Persia, which have been carried five miles or more by the water of insignificant streams. Abich, in his famous Memoir on the "Geology of the Caucasus," mentions how widely spread the erratic blocks are in some of the Caucasus valleys, and notably in that of the Terek. In some cases they have travelled a long distance. But, he continues, "the transport of these blocks has nothing in common with that of the true diluvial phenomena of the European mountains, but is due to alluvial causes which still operate, but in a diminished manner. He describes how, through the configuration of the central boss of mountains, and the disposition of the terraced valleys around the Kazbek, there occur in the upper part of the valley of Deivdareki, or Zach Don, which flows between the Kazbek and Daril into the Terek, very large glaciers, which sometimes have moraines of 200 and 300 feet high. These dam back the waters of the Terek, until they break through and scatter the blocks and other moraine *débris* along the Terek valley as far as Vladeikaukas, ten or twelve versts distant from the mountains. He mentions how similar blocks, from 250 to 300 feet in circumference, have been carried by similar currents from the central mass of Ararat for a distance of seven versts. The largest block he had noticed which had been thus carried in the Terek valley was 300 feet in circumference. [5]

This evidence ought to satisfy any sceptical person of the competence of large masses of water in motion to remove and distribute great rocks, etc., and it must be remembered that all the instances quoted are of mere local and small floods, and quite different, at all events in degree, to the mighty flood which we are compelled to postulate if we are to explain the widespread phenomena of the drift. Such a flood as we contemplate would certainly be physically capable of transporting great boulders over vast distances, of dragging them across the troughs of deep seas, and of thus explaining

[4] Journ. Roy. Geol. Soc. xxxiv. 27-28.
[5] Mems. It. P. Acad. 7th ser. vii. 519-523.

the transport of Scandinavian boulders to Central Russia and the borders of the Carpathians, and the bringing of Norwegian stones to Norfolk. It would explain, as no appeals to ice or to water in ordinary action can explain, the breaking up of great masses of chalk and the removal of blocks several hundred feet long, and their deposition in the midst of stratified and contorted sand and gravel containing shells, such as we find at Cromer and elsewhere. It would explain the curious position in which the boulders so often occur in Sweden and in Germany. In the former country they are frequently found lying on the very tops of the äsar, whose upper layers are stratified and therefore have been deposited by water. In Germany they are found for the most part buried in the great sheets of diluvial sand containing shells as they also are in Denmark.

I may add that the sands referred to from Germany have been examined microscopically, and their grains have been shown to have the rubbed and worn surfaces which sand acquires from being tossed about by water.

The same omnipotent cause would account for the way in which the granite boulders of Shap have been thrust through the gap at Stainmore and then spread over the Yorkshire plains in diverging lines. Nothing but a vast current moving across the English Apennines at this point would, it seems to me, explain the facts, and also explain the absence of Shap boulders from the Lancashire and Cheshire drifts.

This cause again seems to me to be alone competent to explain the diverging and converging lines along which the boulders have been carried, some of them converging on one spot from many directions, as in Central England, some of them coming from different directions, when we often find one line of migrants which must have passed athwart another, as in the case of the boulders of North Germany and Holland. All this can be explained if we postulate a great eruption of water tossed about and driven in various directions, but not by any theory of water acting in its normal way, or by ice.

This cause also accounts for the transported blocks diminishing in size, and becoming more rounded and polished the more distant they are from their original localities. Such would necessarily be the result of their transport by currents,

but when a block has been transported on an iceberg, or carried by a glacier, it is a matter of indifference whether it has been carried one mile or 100 miles, so far as regards the form and dimensions of the block. " If the great majority of blocks," says Mr. Hopkins, " transported from a given locality be rounded and polished, there is a strong presumption that water has been the transporting agent ; if, moreover, the blocks do not exceed a weight of a few tons, the probability of that mode of transport is increased ; and finally, if we find that the magnitude of the blocks generally diminishes as their distance from their original site increases, till at length they degenerate into rounded pebbles, the previous probability appears to me to approximate as nearly to certainty as we can reasonably expect."

This distribution of the blocks according to their gravity has been referred to by Murchison. He says, " The drift of Russia and Germany, in common with that of England, exhibits, for the most part, a diminution in the quantity and size of the blocks the further they have ranged from the source of their origin. Hence in the parallel of Moscow, to which place, and far beyond it, they extend, the fragments of granite and greenstone seldom exceed two or three feet in diameter, while near St. Petersburg their diameter is often as many yards. In passing from the White Sea to Nijni Novgorod, the same facts present themselves. To the south of Ustiug the granitic and greenstone blocks began to be scarce, though, as before said, we met with a fine example several feet in diameter, even as far south as Garbatof, on the Oka." [6]

A great flood of water moving over the country will also, it seems to me, best explain the driftless areas, such as the famous one in the government of Orel, and the other in Wisconsin. We can understand by this means, and this only, how a motive force which was competent to deposit *débris*, and to leave its footmark over such a wide area, would, when its force was more or less spent, have had to move round on either side of any table-land, or considerably elevated tract which it met with, thus depositing its *débris* all round its edges, and leaving the high ground itself bare. The same

[6] Russia and the Ural Mountains, 523-4.

flood of waters would account for the singular infrequency of boulders over large spaces, and their abundant occurrence in others, a feature common on the large plains of Northern Europe. They abound on heights, and under the lee of high ground, where the force of waters would be in a measure arrested, and they would have to drop their loads. Murchison is here again the best authority we can rely upon for *the facts*, the *inference* being our own. " In Mecklenburg and Prussia," he says, " the blocks occur, for the most part, as in Russia, on the slopes or sides of hills, and occasionally in thick groups. The tracts between Schwerin and Lubeck on the east, and again round Seelaw on the right bank of the Oder, being fine examples. In the rapidly undulating country between the Oder and Posen, particularly at the village of Kähorl, the largest boulders are found on the north side of a little hill. . . . In the sandy plains east of Posen, not a block is to be seen for several miles from east to west ; but the moment you reach the small elevations somewhat more argillaceous, which rise towards the Polish frontier, they are again numerous. In that frontier sandy plain the blocks are usually small, but in the hills between Konin and Koláa, the subsoil of which is composed of tertiary claystone as white as chalk, loads of large blocks are buried in and mixed with gravel and sand at heights of 300 or 400 feet above the sea." [7]

The erratics in America tell the same tale. They are not uniformly spread over any area, but are generally abundant in the higher localities, such as the top of Cape Tourment, Lower Canada, 2100 feet above the sea, on the summit of Montreal Hill, and on the high mainland north of Lake Superior, but they exist in greatest numbers along the course of most of the Canadian rivers at all points where the configuration of the land is such as to oppose the greatest impediments to their transport along the river valleys. "This clearly indicates," says Hopkins, "the operation of strong currents and floating river ice in transporting such blocks from the places in which they were first deposited by some more general agency."

It has been remarked again that certain perched blocks,

[7] Id. 522-3.

and others in the hollows of glens, sit upright in their beds with their heavier ends downwards and their broad surfaces vertical.[8] This position shows they were not left by anchored bergs which melted away *in situ*, where they would have lain in the position they were in or under the berg itself—that is, on their broad sides. The fact of their occurring in this way shows they were buoyed up by water and under its influence when deposited.

We have referred to many of the boulders having been rounded and abraded by water, but the opposite condition is just as remarkable. The fact of so many far-transported blocks being found retaining their edges partially blunted, is inconsistent with their having been subject to any long-continued abrasion either by ice or water, and is consistent only with their having been transported by a mighty effort which has taken them up and redeposited them again after a short interval only.

The movement of blocks over heights greatly exceeding the original site of the stones seems also explainable by no other means, as was long ago urged by that most careful and talented geologist, Mr. Cumming. Speaking of the well-known cases in the Isle of Man, he tells us how the granite boulders have been driven over the pass of South Barrule right across to the western side of the mountain. " But the most remarkable circumstance is, that in ascending from the pass to the summit of South Barrule, we fall in with three or four of considerable size, and there is one which I have noted within sixty feet of the top of the mountain, and quite on the western side of it, certainly not less than two tons weight. The summit of South Barrule is in a direct line between this granite boulder and the granitic boss whence it has come, and the difference of height is 788 feet. But there is a slight depression between the granite boss and the ultimate rise of the mountain, across which the boulder must have been transported, viz., that in which the sixth milestone stands, and this milestone is distant about a mile and a half from the top of the mountain. Hence we have a rise of 853 feet in a mile and a half up which the granite boulder ascended to the top of South Barrule, and then slid

[8] Q. J. G. S. vol. 29, lxiii.

down some sixty feet on the other side. Had there been but one boulder, we might perhaps have concluded that it had been carried thither for some purpose by human agency, but the circumstance of there being so many scattered at random all over the surface of the mountain precludes such a supposition." Mr. Cumming, who, it will be remembered, was a strong believer in the glacial theory, then goes on to say: "Here then, it seems to me, we have the evidence of some great diluvial action, an indication of enormous waves with great carrying power sweeping over the surface of the island, and breaking upon the mountain summits. How far the transport of these granite boulders may have been aided by their being frozen (perhaps) into masses of ice, must remain a mere speculation; but I do not see how it is possible, with any conditions of relative sea level, to account for the phenomena here presented to us by any known effects of ice alone, and without taking into the reckoning the agency of some great cataclysm or series of cataclysms. Here, if anywhere, certainly we must have recourse to the theory of great waves of translation proposed for our acceptance by the gifted author of the Silurian System." [9] In this view I most cordially agree, and the desperate efforts which have been made since Mr. Cumming wrote to twist and alter the laws of physics in order to escape this most reasonable and simple conclusion, and all on behalf of an unscientific and even metaphysical devotion to *à priori* prejudice, have only strengthened it still more.

We have seen how difficult, and in fact impossible, it is to account for the perched blocks on the theory of icebergs or an ice-sheet. No such difficulty presents itself with a transporting wave of waters. This wave, as we have seen, would naturally deposit clusters of boulders such as those that occur where the perched blocks are generally found, viz., in the neighbourhood of, or on the flanks of the hills which it met on its course, and these same boulders would themselves be naturally arrested by the points or sharp ridges projecting from the general surface, and as the wave subsided, probably gently, would be left leaning against or perched upon these same arresting projections.

[9] Cumming's Isle of Man, 176-178.

The arguments regarding the distribution of the drift may be supplemented by others from other epochs and other latitudes. Thus Mr. Hopkins, speaking of the diluvial theory of the explanation of the drift, says : " We are too apt to direct our attention exclusively to the phenomena of what is emphatically termed the drift period. It should be recollected that some of the older geological periods furnish us also with records of similar phenomena. The conglomerate of old red sandstone in the Western Highlands presents an enormous aggregation of boulders of considerable magnitude. In the neighbourhood of Oban it is seen in immediate juxtaposition with the erratic blocks of granite. The blocks of the older period have evidently been subjected to very great attrition, and have thus been doubtless much reduced below their original size. Many of them, probably, were as large as the neighbouring blocks of granite. Mr. J. C. Moore also, in his paper on the fossiliferous Beds of Wigtonshire, has described some very remarkable beds of conglomerates containing, among smaller boulders, rounded blocks of four or five feet deep in diameter, and regularly interstratified with other beds. Mr. Griffith has also described Silurian conglomerates in Connemara containing blocks of great magnitude. It would seem extremely difficult to assign the transport of these blocks to any other agency than that of water."[1]

The same conclusion seems to follow from the existence of far-travelled blocks in the Tropics far away from any possible ice action, or from that of any diurnal movement of water as known to us. Thus the hottest region of the earth as known to us is probably British Guiana, yet it was there that Sir Robert Schomburgh discovered a great mass of travelled boulders, looking, he tells us, strikingly like the refuse of a smithy. This is clearly a region, as Murchison says, in which no ice or glaciers can ever have existed.

These facts are paralleled by those observed in Jamaica by De la Beche, to which attention has been already called; by the boulders found in the plain of Upper India, and by those in Persia described by Mr. W. T. Blandford. All these seem to have no other possible explanation than that they have been distributed by water in rapid movement. Again, if we

[1] Q. J. G. S. viii. 29.

go to another district, Mr. Darwin says of the Azores : " At my request Sir C. Lyell wrote to Mr. Harting to inquire whether he had observed erratic boulders on these islands, and he answered that he had found large fragments of granite and other rocks, which do not occur in the archipelago." [2] It is true Mr. Darwin attributes the transport of these boulders to icebergs, but I confess that icebergs in the latitude of Sicily in the Western Atlantic are very improbable things in themselves. Apart from this, how could icebergs mount up on these islands whose fauna and flora show they are at least as old as Tertiary times ? We should need very strong evidence of an arctic facies in the fauna and flora of these detached remains of the Miocene Atlantis, which is not forthcoming, before we could credit the presence of icebergs there. These granite blocks seem to me to afford the same evidence as those above mentioned, not of ice transport, but of a wave of diluvian waters.

This will suffice in regard to the boulders.

We will now consider the abnormal striæ running across plateaux and traversing wide districts irrespective of their drainage lines, which we have seen to present very insoluble difficulties, when we attempt to explain them by means of the theory of ice-sheets, or by any theory of ordinary water action. It seems to me, while we must concede that glaciers cause such striæ in mountain valleys, where they remain in many places as the witnesses of former glaciers, that nowhere has ice been seen, nor can we realize how ice could in any form cause the striæ which traverse wide districts irrespective of their drainage contour. Here, again, we must revert to the teaching of the older geologists, who argued that these lines and scratches, instead of having been produced by ice, were caused by great masses of detritus dragged along rapidly by waves of water pouring over the country, sometimes in a continuous direction, accounting for the straight and parallel character of many of the lines, sometimes slightly deflected by opposing obstacles. That stones moved along by water do produce such scratches seems plain enough, although the number of published observations on the subject is not great.

[2] Origin of Species, 441.

Speaking on this subject, Sir James Hall says: "The only case which has as yet occurred to me, bearing any relation to the action here alluded to, arose from a cause seemingly quite insignificant. In a neighbouring county, a country house, situated upon the slope of a hill, was assailed by a sudden torrent of water, produced by the bursting of a thunder-storm on the hill above. The impetuosity of the stream was such that it forced its way through the under storey of the house, carrying along with it quantities of sand and gravel, and stones of considerable bulk. Happening to be upon the spot a few weeks after the accident took place, I observed that every stone as it passed through the house, had left a rut or scratch behind it upon the flags over which it passed." [3]

Again, in mentioning the removal of a large block of stone four or five feet in diameter on the coast of the Murray Frith, during a stormy night in winter, to the distance of ninety yards, he adds: "The course of this stone was marked upon the sand below by a deep and broad furrow, which remained visible for a long time afterwards." [4] If Sir James Hall be deemed an old authority, although he was a very experienced one, it may be well to quote a more modern one.

Mr. Koch, in a discussion upon Mr. Mackintosh's paper, read on March 12th, 1873, said that " in the late floods in Bohemia, blocks as large as six feet by four had been transported by the rivers, *and their surface had been striated during the pro_ cess in a manner much like that produced by ice.*" [5]

Murchison said that in one of the valleys of Glaris, which he had walked through with M. Escher von der Linth, they observed a mass of glacial *débris* containing striated stones, etc., give way and cause very numerous vertical striæ on the rocks precisely like those attributed to glaciers. [6]

Mr. Mallett argued that many of the so-called glacial striæ were caused by loose material slipping and sliding down declivities, even very small ones, and thus scratching and furrowing the supporting surfaces of the rocks in a way he believed it impossible to distinguish from the traces left by similar movements of masses of ice ; and he adds the very interesting fact that he and Dr. Oldham once found a large

[3] Id. 182, note.   [4] Id. 157.   [5] Q. J. G. S. xxix. 360.
[6] Bull. Soc. Geol. de France, 2nd ser. vii. 631-632.

sub-angular boulder in deep clay, stopped in the very act of making an uncompleted groove, and under conditions that forbade any supposition of ice action, or any other source of movement but that of the quasi-fluid motion of the whole mass of clay carrying the boulder with it.[7]

These instances show that such striations not only can but have been produced in certain cases by detritus rapidly impelled along by water. As I have said, I do not for a moment question the glacial origin of many of the striæ in mountain districts, where glaciers doubtless formerly existed and acted as glaciers act now, but I absolutely refuse to believe, in the teeth of the evidence, that ice is capable of traversing wide districts irrespective of their contour and causing striations upon them. It is plain on the other hand that gravel and detritus moved by water in flood would be capable of such work. On this issue I will again quote Mr. Hopkins. After speaking of striæ as having a divergent direction in mountainous countries, he speaks of the probabilities in these cases being that the striæ were due to glacier action, and he goes on to say : " but after having escaped from the valleys on the immediate flanks of the central mountains, what cause can have operated to drive forward through the more open sea the masses of material which may have been the striating and grooving agents in the same continuous direction, and with such a force and determination, that they could not be turned aside by the numerous projecting bosses of solid rock on which they have so effectively engraved the record of their transit ? . . . It appears to me, that we are driven to the alternative either of rejecting all theory on the subject, or of adopting that which would attribute these currents to *waves of elevation* resulting from frequent, sudden, but not extensive, vertical movements of the central range of elevated land. . . . Whatever may be the nature of the blocks, they become almost universally smaller and more rounded as we approach the external boundary of the drift. This seems to me conclusive as to the nature of the transporting agency in this outer zone. I can conceive water alone to be capable of giving these characters to the transported materials."

This conclusion is by no means so isolated as might at first

[7] Q. J. G. S. xxix. 224.

appear. As I have tried to show in the earlier chapters of this work, it was accepted as explaining the same phenomena by a large body of the most experienced and careful observers that geological science has produced, and it remained the creed of the orthodox in geology until the glamour of Lyell's teaching diverted men's minds into the paths of metaphysical and *à priori* reasoning. Not only so, but the necessity for invoking the movement of great masses of water is conceded at this moment by many strong glacial advocates, who cannot disguise the character of the facts which have to be explained, but who qualify their admissions by attributing them to transcendental causes, in the hope that they thereby may find some way of appeasing their Uniformitarian conscience. I will not quote from European authorities whom I have already referred to at some length in former chapters, but will rather do so from the pages of one or two American geological veterans, one of whom has been himself the champion of some of the extremest developments of the Glacial Nightmare, and whose concessions in this behalf may therefore be more widely accepted : I mean Professor Dana.

Professor Andrews, speaking of the gravel deposits of the United States, says : " These gravel hills are often sharp and conical and interspersed with deep circular valleys without outlets, from which the region has obtained the popular name of the Potash Kettles. . . . It would seem to be an unavoidable inference that our drift of this region not only came from the north, but it came in a vast sweep of water deep enough to cover gravel hills 800 feet high, and with velocity enough to throw such coarse material into lofty and steep summits."

Dr. Hildyard who has described the widespread deposits known as the Orange Sand Formation in Mississippi and elsewhere, says he attributes them to a great inundation of water apparently devoid of organic life.

The most notable witness, however, is Professor Dana, with whom on this issue I only feel disposed to quarrel when he attributes the stupendous flood to which he appeals to so inadequate a cause as the melting of an ice-sheet. In regard to the reality and vast proportions of the flood in America, he speaks in very plain language.

He is constrained by the facts as they present themselves there, to say of the superficial deposits : " 1. The prevalent stratification of the old terraced alluvium over New England is evidence of its sedimentary character; 2. From the vast width of many of these alluvial regions, we infer an extraordinary flow of waters over the country; 3. The great thickness of the deposits, rising in some places for long distances to 200 feet or more above the river, and no doubt originally filling the valley to the level of the upper terrace, and still more the frequent occurrence of thick obliquely laminated layers—one such in the New Haven region reaching the extraordinary thickness of eight feet, are indications of a very rapid and abundant supply of sand and gravel; and the beds of coarse stones, often intermingled, tell of currents of immense power, or of sudden falls from the floating or overhanging ice; 4. The vast flow of waters and the vast flow of sand and gravel were concurrent events. 5. The extent of the floods throughout New England, is proved by the extent and character of the deposits, which also indicate that the melting of the glacier did not occur only at the southern margin, so creeping slowly northwards, but simultaneously over its wide extended surface. . . . About the head of New Haven Bay the terrace plain has a height of from forty to fifty feet above the sea. It is everywhere stratified, and the beds are generally obliquely laminated. The laminæ of the obliquely laminated layers dip southwards, that is, rise to the northward, and thus show that they were deposited under the action of currents from the south. . . . But at the entrance of the river valleys, or where the river valleys merge into the region of the New Haven plain, at a level about twenty feet from the top of the deposits (and twenty to twenty-five above the present sea level), there is an abrupt change in the direction of the oblique lamination, the layers above this level rising *to the south* instead of to the north. . . . That the waters of this flood came loaded with sand and gravel in enormous quantities is indicated by the thickness of the obliquely laminated layers; that there were plunging waves in the estuary connected with both the incoming tide and the flood, is made manifest by the composite character of these layers." [8]

[8] Dana on the Glacial and Champlain eras in New England, *passim.*

On the geology of the New Haven district, he says : "When it is considered that the waters which levelled this plain were the same that distributed the sand and gravel of the drift formation, that, in other words, the plain is only the upper surface of the drift formation then deposited, it is obvious that the waters, to have made such a slope over so wide a region, even to the shores of the bay, must have been those of a flood of no common magnitude. For the last mile, the flooded waters of Mill River were united in one great tumultuous sea with those of western Hamden, or those of the several tributaries of Wilmot Brook, for the plain in this part has one level all the way across, a distance of three miles. Such a flood . . . must have been simultaneous with the deposition of the material arranged by the waters." And he concludes, *inter alia*, " that the formation of the northern part of the plain beyond fifty feet in elevation, was due mainly to the floods of fresh water filling the valleys and spreading widely over the plains." [9]

Dana again presses this teaching home in his Manual. " The fact," he says, " that a flood vast beyond conception was the final event in the history of the glacier (i.e. of the great American ice-sheet) is manifest in the peculiar stratification of the flood-made deposits, and in the spread of the stratified drift southward along the Mississippi valley to the Gulf, as first made known by Hildyard. Only under the rapid contribution of immense amounts of sand and gravel, and of water from so unlimited a source, could such deposits have been accumulated." [1]

In regard to the portentous dimensions of the flood, he urges that the Connecticut River, whose ordinary modern limit is from twenty to thirty feet high, then discharged 1·2 cubic miles of water per day, or from 330 to 440 cubic miles per annum, and in order to supply the water he postulates that from 359 to 478·5 cubic miles of ice were melted annually, or more than a cubic mile per day in the drainage system of this one river.[2] These are the extravagant demands on our credulity made by a great geologist, who cannot deny the vast diluvial effects, but shrinks from an appeal to an adequate

Id. 95-98.   [1] Op. cit. 553.

[2] Dana on the Flood of the Connecticut River, American Journal of Science, xxiii. 367, etc.

cause, and shelters himself behind a transcendental one in order to avoid what seems to be a quarrel with the Uniformitarian views of orthodox geology. I prefer the courage of another great geologist, in some respects the greatest geologist who ever lived.

"A vast portion," says Murchison—"by far the greater part—of this drift has, therefore, we think, been transported by aqueous action, consequent on powerful waves of translation and currents occasioned by relative and often paroxysmal changes of sea and land. Now that we are sustained by the reasoning of mathematicians, who show us that with sudden vertical elevations, each not exceeding fifty feet in the case of an ocean of 300 or 400 feet in depth (and might not corresponding depressions produce the same?) bodies of water have the power of hurling on enormous blocks, sand and gravel to vast distances and over considerable inequalities, we are relieved from one of the great difficulties opposed to the rational explanation of the position of a very large proportion of this drifted material. Whatever may have been the period of their action, such aqueous debacles have probably formed many of the conglomerates of previous ages, and with the help of ice floes, much of that foreign drift of which we have already treated." [3]

I have now surveyed the problem as illustrated by the drift phenomena with such care as I can command. I have tried honestly to face the various difficulties presented by them, and have concluded as an induction from the facts themselves that the distribution of these complicated beds can only be explained by a flood of waters, a great and rapid flood of water which passed over the country, transporting great boulders in its mighty embrace, and laying down the soft deposits as we find them laid down in a large part of the northern hemisphere. I know of no other hypothesis which will explain all the facts, and which is therefore a valid hypothesis. This conclusion is exactly what I have maintained in a previous work on many other grounds, and I have, therefore, no hesitation in accepting it as the best tentative theory available. It involves our discarding a great deal of the exaggerated reasoning of recent Ultra Glacialists, and

[3] Russia and the Ural Mountains, i. 553.

our abandoning much of that extravagant form of Uniformitarianism to which geological reasoning has latterly been wedded.

On the other hand, it enables us to bring under one common law the phenomena presented by the biological and archæological records with a large body of facts presented by stratigraphical geology, and above all it enables us to solve a great problem without ignoring a single fact, without fencing with a single difficulty, and without having recourse to arguments and to logic which belong to other domains than that of science.

THE END.

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